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STUDIES IN THE PSYCHOLOGY OF TOUCH.

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I. Psychology of Touch in General.

If the nervous mechanism of the body could be viewed, apart from the rest of the body, with all the nerve trunks and branches *in situ*, the form of the body would be duplicated in its externality by the network of nervous fibres and filaments which ramify and penetrate every part of the surface of the body. It is believed that each one of these fibres or filaments, however fine, connects directly with the nervous centre, and that whatever stimulus is received by each is carried to the centre over independent fibres, and that no transition of stimulus from one fibre to another is possible, however closely they may be placed together. Not only is the isolation thus perfect, but if a fibre be broken or cut, there can be no further communication established between the part of the surface where this fibre is attached and the nervous centres until the distal portion is regenerated. If the nerve trunk be submitted to mechanical pressure, the communication is severed so long as the pressure continues. Thus the whole surface of the body is in more or less complete independent connection at each point with the higher centres, and, as we shall see, this independence in structure is necessarily followed by a similar independence in function, whereby the great differences in local discriminative ability have arisen and the conditions are furnished for further dermal education.

One of the first things to notice and probably the most striking fact in the study of the skin as a sense organ, is the great difference in its extent as compared with the other organs of sense. If the sensitive surfaces of all the other sense organs were spread out in direct contact with the outer or objective world, the whole of these taken together would scarcely equal the surface of the palm of the hand. Though of course this comparison must of necessity be crude, for each organ's sensitiveness is measured by wholly different stimuli and these are incommensurable with respect to each other. Still when it is called to mind that eye, ear and nose, with all their special adaptation, are but "specialized dermal cells," the comparison is not wholly without value. Furthermore, embryology teaches that not only are the sense organs thus developed from the skin, but that the whole of the brain and the spinal cord are infolded and developed ectodermic cells. The influence of this fundamental connection between the higher centres and the skin, may be much more far-reaching than psychology has yet thought. There is at least a suggestion of the utmost importance in the philogenetic relation of the skin and the nervous centres. Some have dared to hint that the objectivation of the subjective states may here have a real causal basis. But it is safer to wait until biology, at least, has more to say on this point, before making assertions the proofs for the truth or falsity of which are still hidden in the abyss of our ignorance.

Then, too, the skin is situated and conditioned unlike any other of the sense organs, in that it is turned in all directions. The other sense organs are more or less directed toward the front and at all times are limited to a part even of their immediate environs. In a large measure this limitation is not true of the skin, though of course certain accommodations of hand and body are habitually made to facilitate and refine perception through touch. As a sense organ the skin is much less specialized than any of the other sense organs. Its range is not only larger, but it takes cognizance of the more fundamental properties of the material world. Our eyes are useless in the dark, our ears are without value when there are no vibrations in matter, but the conditions of touch remain so long as there is objective existence at all. The skin is the mother sense and out of it, all the other senses have been derived. The biologists tell us that the eye is only a developed sensory spot of the skin, and Preyer [1] even goes so far as to say that as a basis for the fact, we are constantly speaking of the qualities of things seen, in terms of qualities only felt, such as a

[1] Pflüger's Arch. 1881, p. 75.

warm color, a cold shade, etc., that the sense of sight was developed out of the sense of temperature.

If we take the amœba or any of the simpler forms of life, we can readily see that their whole life must depend on the kind of reactions they are prompted to make through skin perception. The whole surface of the bodies of such organisms presents to their environs a sort of diffused touchiness, apparently not more differentiated in one place than another. They are all eye if they see at all; all ear if they hear, all nose if they smell. Wherever they touch a particle of exterior matter, they need no further adjustment to know it as far as it is possible for them to know. There is no parallel to this in any of the other senses. As Aristotle[1] has said, we can have an animal without eyes, without the sense of smell, or taste, or hearing, but all must have the sense of touch. In fact, it is only the few in comparison that have any other sense than touch. With this primordial and far-reaching notion of touch in mind, let us examine the properties of bodies known to man through the senses. Perhaps this can be more quickly done by excluding the properties of objects derived from or through the other senses. Through the eye we get color and motion and perhaps an indistinct (until corrected and helped by the other senses) notion of distance. For the ear only sound, or vibrations within certain limits. For the nose odor, and for taste certain chemical stimulants. All the rest of our mental lumber has come through the skin either directly or indirectly. If we were asked to name the most permanent and most important properties of bodies, we should not go out of the realm of touch, including the so-called muscle sense, which is largely skin sense. We would name hardness, weight, temperature, tenacity, compressibility, shape, roughness, etc. It is easy to see that these are the fundamentals. We can easily be deceived in color, but not so with hardness. Certain interpretations of odors are very largely due to the personal factor, but weight does not exist as a variable except in degree. We have standards fast and exact for determining weight, resistance or cohesion of bodies, but not for the strength of their odor or the shades of their colors. We do not speak of a person as abnormal if his ear is not discriminative for great difference in tone, but we consider him abnormal if he could not distinguish thousands of variations in location, in quality, in temperature, etc., of objects through touch.

Then again it is perhaps helpful to see that the other senses depend largely on the skin for proof of their assertions as to

[1] Psychology, Wallace's Trans. p. 67.

the more important of their activities. We come to judge of form by the eye, but we are in constant need of tactual measurements to correct and prevent misconception. Doubting Thomas thus becomes a sort of type for realists, all of whom insist on the most certain proofs and so rely on the tangible. As Mr. Fraser [1] says, in his suggestive essay, "In the case of the unreflective but practical thinker the question, 'What do you mean by a real world?' is openly answered and without bias by saying, 'It is a world that we can *touch*.'" "He can be persuaded that the object he *sees* before him is illusory; but if he is allowed to stretch forth his hand and can touch it and feel it there, the last remnant of doubt as to its real existence will have fled." Very rarely, if ever, do you see the fakir or sleight-of-hand performer practicing his tricks save to deceive through the special senses. Indeed, the phrase "sleight-of-hand" may have for its earlier psychological basis, the meaning now best rendered by slighting of the hand. Dr. Johnson's reply to the Berkeleyan philosopher who pressed him with his idealism, was, as everybody knows, "Throw a brick at him."

This fundamental and deep lying significance of perception through touch is seen in the commonest words having for their import to know:—*perceive* means to take thoroughly or to take hold of; *conceive* has for its fundamental and original meaning, to take hold of things together; *apprehend* is to catch hold of; *comprehend* means only to catch hold of more than one thing; *understand* means measurement of things by letting them rest on you; we *accept* a thing mentally when we take hold of it. The modern slang phrase "catch on" illustrates this same tendency to coin terms designating intellectual activity. And if the theory be true that slang is a linguistic atavism, this last phrase is full of psychological significance. Just what the full meaning of this strong tendency is, perhaps does not fully appear, yet it is certainly significant in preserving an early and for that matter an inherent theory of indisputable knowledge. "Why touch," says one writer, "the simplest and earliest sense, should in its higher forms be more than any other sense associated with the advance of intelligence, is due to the fact that tactual impressions are those into which all other impressions have to be translated before their meaning can be known. It is certainly true that a highly elaborate tactual apparatus comes to be the uniform accompaniment of superior intelligence." As has often been said, the skin is the boun-

[1] Psychological Foundation of Natural Realism, AMER. JOUR. PSYCH., Vol. IV. No. 3, p. 429.

dary between the ego and the non-ego; that all that is within is subject, and without is object. So in touch we come nearer to the "thing in itself," if you please, than through the medium of any other of the senses. We can not smell a *thing*, but only the gaseous particles given off by the *thing;* we can not come into contact with a *thing* through vision, but only with the vibrations of ether which are radiated *from* the thing; in taste pure and simple, we are made cognizant only of a thing after its dissolution, and not of the thing as a whole. So there comes to be less need of interpretation in the case of skin perception than in any other, and so less opportunity of illusion. Speaking of this point Sully [1] says: "Tactual perception in so far as it is the recognition of an object of a certain size, hardness and distance from our bodies, involves the least degree of interpretation and so offers little room for error; it is only when tactual perception amounts to recognition of an individual object, clothed with secondary as well as primary qualities, that an opening for palpable error occurs."

Herein lies a pedagogical principle far too infrequently used. In the years of childhood we feel the world of sensitive appearances nearer to us; we live immediately with and in it; there is an intimate bond of living dependence which unites us to it, and which is only broken when we are removed from contact with things. The brook to the boy with his pantaloons rolled up and the water gurgling around his legs is far more a brook, far more real, than to the poet who only hears its gurgle, or sees it sparkle. This close and intimate relationship between subject and object which we get through touch has led men to say that if the facts of physiology were rightly interpreted, they would lead irresistibly to the conclusion that sensation is not confined to the brain, but is spread over the whole sensory system. Wherever there is a sensory nerve there may be a sensation.

Johannes Müller said: "We do not assert that the mental principle has its seat in the brain alone. It is possible for the mind to act and receive impressions by means of an organ of a determinate structure, and yet be present generally throughout the body. The mental principle or cause of the mental phenomena, the conception of ideas, thoughts, etc., can not be confined to the brain. It exists, though in a latent state, in every part of the organism." Or as Isaac Taylor said: "The mind is not, as we suppose, the prisoner of the attic story, but is the occupant at large of the entire animal

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[1] Illusions, p. 49.

organization, acting in each part of the structure according to the purpose of each."

If we consider carefully the probable value of double cutaneous sensation to a child in giving to it the basis for a separation of the ego and non-ego, we shall see without doubt that this simple and oft-repeated experience is of far-reaching significance. When a child first takes hold of something external to itself, if there be but a single sensation or set of sensations fused, there would not of necessity grow up a notion of otherness. But it seems to me, this basis for otherness has its origin in the double sensations it receives when it touches and handles its own body. If it takes its foot in its hand there are two wholly different sets of sensations presented simultaneously, and because of their unlikeness there must come a feeling, or consciousness of otherness, which in no wise could come through any set of single impressions. Later in life we come to use the special senses more and more and the skin sensations are left to form the greatest part of the semi-conscious state. When we stop thinking, and just *feel*, we find ourselves in a flood of sensations coming from the skin. While it is in a measure true that we are accustomed to neglect all of these sensations, notwithstanding they are pouring themselves in upon us and in no small measure constitute the mood, affect the temperament and put the attention in a state of uneasiness which demands and requires more stimulus to prevent mind wandering than when the subconscious is minimized. "We are most fully conscious," says Bastian, "when we are most receptive of external impressions, and we lapse into a completely or partially unconscious condition, when the advent of such impressions is for a time prevented, or when we are intensely absorbed in some train of thought, that is, when the activity of other portions of the cerebral hemispheres in some way dwarfs or eclipses that of the sensorial regions proper."

It is, to say the least, an interesting notion of Lotze [1] that whenever we bring a foreign body in contact with the skin, the consciousness of our personal existence is prolonged into the extremities and surfaces of this foreign body. According to him a tall head-dress is worn preferably because it lengthens our own "personality," producing the pleasing illusion that we ourselves extend up to that point. Thus we can understand the "disposition in low stages of culture, perfected afterwards in higher ones," to wear plumed helmets, waving and lofty coiffures. These fortified the consciousness of the wearer with the feeling of a majestic upward ex-

[1] Microcosmus. Eng. Trans. Vol. I. p. 592 et seq.

tension of his personality, as well as served to increase the fear-inspiring or respect-inspiring effect of the figure on others. So, too, high heels and stilts, while they raise us off of the ground, do not apparently separate us from the ground. The cane to the blind man becomes merely a prolongation of his arm and the touch at its end renders him directly conscious of the distance and quality of the object with which it comes into contact. The second class of these feelings is derived from all hanging and waving drapery and ornament, which "causes us to feel as if we were ourselves present" in the floating, flapping folds and streamers. This notion, he thinks, is the key to the uses of sash and ribbons, girdles, bracelets, trains, and all manner of wearing apparel of this sort. Thus the light gauzy stuff with which the maiden drapes herself is intended not only for the graceful curves that touch the skin in a few points, so as to charm others, but she herself feels that she is present in all of these movements which she "distinguishes as to the breadth, lightness and softness of their sweep." To Lotze all this was not mere theory, but a fundamental law of dress. For he says he has performed for the exact science of dress the same service as Kepler did for astronomy.

Then again, perhaps much of our æsthetic life has a closer connection with the skin sense directly than we are wont to think. There is without doubt truth in Lotze's[1] idea, when he says: "Accordingly it [the vesture] serves to make obvious to the senses the peculiar mobility of the figure and likewise gives to it, by means of the arrangements of its hanging folds, a semblance of the feeling which the figure itself preserves through its surroundings and its carriage, and in which it delights." And again he says in the same volume: "The pose of the figure in plastic art will always have for its principal purpose to show not only the complete adaptability with which the body obeys the spiritual impulse, but also that with which every single part by corresponding counterpoise, tension or relaxation compensates for the displacement of every other, so that the body appears as an extremely sensitive and elastic system of parts which keeps itself in equilibrium."

It is a noteworthy fact that illusions and hallucinations of the insane come through the sense of touch far less frequently than through sight or hearing. According to Griesinger,[2] in 177 cases of hallucination, occurring mostly in sane people, the following relations occur in the senses: Sight alone, 78;

[1] Outlines of Æsthetics. Trans. by Prof. Ladd, p. 76.
[2] Mental Diseases, p. 81. et seq.

sight and hearing together, 46; hearing alone, 16; sight, hearing and touch together, 4; with touch alone, only 9. He adds that diminution or complete suppression of the sensibility of the skin to impressions of temperature or pain is by no means frequent, still less is it general in insanity. Snell found that in 180 patients less than a score were anæsthetic, and where anæsthesia was present it occurred in cases offering very little hope of recovery. "When we read the history of trials for witchcraft," says Dr. Michéa,[1] "we observe that the inquisitors attached a high value to the existence of *cutaneous anæsthesia* as a sign of demoniacal possession. When an individual was charged with the alleged crime, the experts, after having bandaged the eyes, passed a magnifying glass over all parts of his body, previously shaved, with a view of discovering the marks of Satan (stigmata diaboli). The slightest spot on the skin was probed with a needle. If the puncture did not cause a painful sensation, if it provoked no cry or movement, the poor creature was a sorcerer and condemned to be burnt alive. If, on the contrary, he felt the wound, he was acquitted."

The fundamental character of the skin perceptions appears in a curious and interesting way in the Law. It is a well established fact that "mere words will not make an arrest." That is to say, the officer must touch the person whom he wishes to arrest in order to make a binding arrest, unless in some way the person, upon whom the writ is served, submits and shows himself subject to the authority of the officer without being touched. This will be made clear from the following decisions: "Bare words will not make an arrest, but if the bailiff touch the person, it is an arrest and the retreat a rescuous. On a motion for an attachment against three persons for a rescuous of a person taken in execution, it was objected that there had not been a legal arrest, as the bailiff had never touched the defendant. The court said this is a good arrest; and if the bailiff who has a process against one says to him when he is on horseback, or in a coach, 'You are my prisoner, I have a writ against you,' upon which he submits, turns back or goes with him, yet it is an arrest because he submitted to the process; but if instead of going with the bailiff, he had gone or fled from him, it could be no arrest unless the bailiff had laid hold of him. . . . . It is not necessary to show the warrant, or to tell at whose suit you arrest him, unless he demand it. And if you have two warrants in your pockets against him and produce neither, if he be rescued, either party at whose suit the warrants were made out may bring

[1] P. Gray. "Chirurgia," 1609, lib. VII. c. 10 (quoted from Winslow).

an action against the rescuers." In another old case,[1] it was decided: "Genner, a bailiff, having a warrant against Sparks, went to him in his yard, and being at some distance told him he had a warrant, and said he arrested him. Sparks having a fork in his hand keeps off the bailiff from touching him and retreats into his house. And this was moved as a contempt. It was held: "The bailiff can not have an attachment, for here was no arrest nor rescuous; bare words will not make an arrest, but if the bailiff had touched him, that had been an arrest, and the retreat a rescuous, and the bailiff might have pursued and broke open the house, or might have had an attachment or a rescuous against him; but as the case is, the bailiff has no remedy but an action for the assault, for the holding up of the fork at him when he was within reach is good evidence."

Again:[2] "I have not been able to find any real conflict between English and American authorities as to what constitutes an arrest. By all the authorities, a person may or may not be arrested without a manual or actual touching by the officer. Bare words alone will not make an arrest if the party resists the arrest." Many other cases might be cited, but so far as can be determined the same principle is held to in all of them, viz.: "The question as to whether or not an actual physical caption or manual touching of the body of the prisoner is necessary to effect an arrest depends upon the further question, whether or not the arrest is submitted to. If the party resists or endeavors to evade it, there must be an actual touching of his person, or some physical restraint of his liberty to depart, in order to complete the arrest; mere words will not do in such cases."

The importance of this whole matter for psychology depends upon whether touching, as used in arrest, is a survival of the attempt to take the prisoner by force, or whether it be that touch is here recognized as that sense through which errors or misunderstandings are less liable to occur than through any of the other senses. To consider it as a survival of the employment of physical force, seems by far the more natural explanation, and at first thought there seems to be scarcely any other way of looking at it. But the question comes, how would an officer arrest a blind and deaf man, or even a deaf man who could not read, except he make known the arrest through touch? This sense is never lacking completely during normal conscious life and so is always an open avenue through which the outer, or objective, becomes inner or subjective.

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[1] See Genner *v.* Sparks; 1 Salk. 79.
[2] Searls *v.* Viets, 2 Thomp. & C (N. Y.), 224, Potter, J.

However, if it be true, as seems probable, that the touch now recognized in the law of arrest is a survival of a more forcible means of taking possession of the prisoner, its present signification is entirely different, and I am assured by high authority that it is used now to simply make known to the one arrested that he is now in the power of the officer, and there is no means of denying it, because he was made acquainted with the fact through placing his hand on him.

The *rôle* that touch plays in the expression of the emotions of love and friendship can scarcely be over-estimated. The psychological significance of this fact is seen in many words we use for friendship and love. The words, *attraction*, *affection*, and the phrase, *attached to*, illustrate this. As is readily seen, these words have for their fundamental meaning the notion of being bound in contact with the loved one, and so indicate that there is a desire in us for actual dermal contact in the expression of love and friendship.

Dr. Brinton, the eminent "Americanist," has called attention to the fact that the same fundamental significance of the words for friendship and love is shown in many of the Indian languages. With the Chipeways, for example, the root for the word love is *sak*, which means *to attach, to fasten*. The root for the same word with the Aztecs was *zo*, which means *to fasten* one *with* or *to* another.[1]

It needs no exhaustive research to see that with all peoples, their heartiest salutations and greetings, their deepest and most fervent feelings of love and friendship require tactual expression. It has been the custom at all times and among all peoples to shake hands, to embrace, to kiss when friends meet. True it is that these tactual expressions have taken a great many forms, but each of them makes use of some form of contact.

It would be a most interesting study to trace the racial and local variations of these tactual salutations with especial reference to their development and evolution. It seems to me that a careful study of these forms would not only indicate the gradual changes and development of such feelings as they express, but that these accompany the development of finer capabilities of dermal sensitiveness.

The Feugians and Australians hug each other when they meet. Among the ancient Jews this same custom prevailed, supplemented by a kiss. We read in Genesis XXXIII, 4, that "Esau ran to meet him (Jacob) and embraced him and fell on his neck and kissed him and they wept." One of the most universal customs among the East Indians, and the

[1] Essays of an Americanist. (Philadelphia, 1890), p. 410.

Pacific Islanders in general, is that of "nose rubbing," as the travelers are wont to call it. These peoples, when they salute, have the general custom of touching their noses together and perhaps snuffing or smelling of each other. The kiss seems not to prevail so extensively among peoples of a lower stage of civilization in their salutations as it does in the higher stages. The Orientals, as is well known, have an elaborate system of salutations, in which the kiss plays an important part. Indeed, this method of salutation appears to have been practiced from the earliest antiquity by Semitic and Aryan peoples. If we should ask ourselves why we "shake hands," the answer, it seems to me, is not found in the usual explanation that we then see the one whom we meet does not carry a dagger, but in the demand for tactual expression of friendship. It is almost amusing to see to what inconveniences we will submit ourselves merely to grasp the hand of a friend and in return experience pressure from his hand.

Almost innumerable citations might be made showing how the forms have varied, but those given are sufficient to illustrate the point in question. Although, as has been said, the chief purpose of these forms of tactual salutation is that of the expression of the emotions, yet the fact must not be overlooked that a mere grasp of the hand often furnishes us with data concerning the character of a person not obtainable in any other way. The complex sensations of warmth, of firmness of grasp, of passivity, etc., etc., which we derive from a handshake, in no small way affect our opinion of a newly made friend. With some, these dermal sensations are all powerful. A friend related to me an instance which came under his direct observation, in which a young lady refused to continue her acquaintance with a young man simply because, as she said, the touch of his hand gave her "the horrors." It seems that with each of us there is a certain delicacy of touch, a certain degree of warmth, a certain amount of pressure, best suited for personal satisfaction, and corresponding to our notion of the best and fittest.[1]

[1] Since the above was written, I have found that Dr. Brinton has expressed the same idea in his recent book, "*The Pursuit of Happiness*" (D. G. Brinton, Philadelphia, 1893, p. 131). He says: "When we consider how slightly most sensations of touch excite subjective states of mind, it is remarkable that in response to one stimulant, they are among the most powerful known in nature. This stimulus is that of another personality. The most positive feelings of both aversion and attraction are those excited by physical contact of the naked flesh. This is why it has been accepted in so many countries as a sign and proof of amity. The savage Africans touch noses and the civilized European shakes hands or kisses the hand or the cheek.

II. Education of the Skin with the Æsthesiometer.

The important discovery of E. H. Weber concerning the great local differences in the discriminative ability of the skin for the compass points, marked an epoch in touch psychology. His work not only stimulated to careful anatomical studies in order to know more of the skin as a sense organ from the anatomical side, but it has led to studies whose object has been to determine the possibilities of education in the discriminative ability of the skin.

A. W. Volkmann[1] found that the distance between the compass points, placed at the minimum distance they could be felt as two on different parts of the body, could be greatly decreased for any given part by exercise. And not only did this education show itself with regard to the special territory exercised, but strangely enough the symmetrical part on the other half of the body was likewise very markedly increased in fineness for this spacial discernment. He found that this increase, however, was soon lost for both sides, when the exercise was given up, and that after a month's rest, the increase was wholly lost. Suslowa observed that if the part of the skin lying between the compass points be stimulated by means of an induction current, or some light mechanical stimulus, such as touching it with a pencil, the sensitiveness for localizing was increased. Funke[2] found, just as Weber did, that on the median lines of the body, the skin is not so discriminative for the compass points. He also found that, after a month's exercise of a portion of the skin on the median line between the shoulder blades, there was but little or no increase in the power to discriminate between the compass points.

This ability to localize, however, varies greatly with different people. Valentine found that corresponding points on different people varied, even to a difference of four-fold, as regards their ability to distinguish the compass points as separate. But he also found that the ratios between the numbers expressing the fineness of discrimination for different parts of the skin of one person was about the same as that of another, even though much absolute difference existed in their abilities to discriminate finely.

Speaking in a general way, the more mobile the part of the body, the finer the discrimination it possesses. According to Vierordt, the relative fineness of the sense of locality of a given point of the skin of a part of the body, is a function of

[1] Berichte d. Sächsischen Gesellschaft d. Wissenschaften, p. 38. f.
[2] Hermann's Handb. d. Physiol. III. ii. p. 38.

its mobility depending on the relative extent of the excursion which it makes in the movements of the parts concerned, around its own axis, and increases proportionally with its separation from the axis upon which the part turns. While the facts do not support this so-called law completely, it must be regarded as expressing in a general way approximate truth.

In order to test the work of Volkmann and others, I began and carried out a series of experiments with special purpose of determining (1) how rapid the increase in sensitiveness through practice would be under given conditions; (2) to what extent the transference of this increased power would be made; (3) whether the sensitiveness of the surrounding parts would be raised or not; and (4) the curve for the loss of the power thus gained, after the exercise of the part had ceased.

METHOD OF WORK.

The tests were made on two subjects, F. B. D. (a man) and C. W. D. (a woman). For F. B. D. an area of the skin 7 cm. square on the palm side of the lower part of the left arm, beginning about 5 cm. below the elbow, was selected; while for C. W. D. a portion of the skin of the right arm 5 cm. square, also on the palm side, half way from the wrist to the elbow, was taken. In making the tests an æsthesiometer was used, whose points were of ivory and moderately sharp. It will be seen, by referring to the table, that two sittings for each subject were taken daily (with the exception of a few days), practically at the same time each day, and extending over a month. In the beginning of the work both arms of each subject were tested carefully on equal symmetrical parts and the discriminative ability noted for each. After this ability had been determined for both arms of each subject, experimentation was confined to the right arm for C. W. D. and to the left for F. B. D. At each sitting from twenty-five to forty observations were made on each subject. The subject being blind-folded, the skin was touched with the æsthesiometer, and the subject asked to judge whether one or two points of the instrument were touching the skin. The operator was careful to present these two conditions indiscriminately and equally often. The subject was so situated that the arm was always at rest and not otherwise stimulated. The operator was careful when two points were presented to have them touch the skin as nearly as possible at exactly the same time, otherwise the discriminative power would be reduced to that of the time-sense for

impressions, or at least so confused with it that the results would be too complex and varied. Then, too, care was needed to so shift about over the part to be stimulated that the after-images would be as little as possible confusing. To this end it was necessary to work often quite slowly and not to make too many observations at one sitting. Besides, the effect of fatigue was plainly noticeable after a long series of observations and so it was important to notice this factor. The method of finding the exact symmetrical part on the arms consisted in inking afresh the boundary lines of the spot exercised and then, after placing the palms of the hands together, symmetrically pressing the arms together. Thus the same sized area and exactly symmetrically placed was obtained. The same plan was used at first so as to obtain the relative ability of the two arms before either had undergone any special exercise.

RESULTS: The following table is a statement of the daily ability to discriminate, with the date and time of day given. The results are given in mm., and in all cases represent the distance between the two points of the æsthesiometer, where at least seventy-five per cent. of the judgments were correct. It will only be necessary to glance down the columns to see the great increase in the discriminative ability. As will be seen for C. W. D. the distance of the points to begin with was 21 mm., but the average distance of the last week was only 4 mm. For F. B. D. at the beginning of the work, the points must be separated 33 mm., while the average distance for the last week was only 3 mm.

TABLE I.

DATE.	C. W. D. A. M.	C. W. D. P. M.	F. B. D. A. M.	F. B. D. P. M.
October 11,	22 mm.	24 mm.	29 mm.	26 mm.
" 12,	19 "	20 "	33 "	15 "
" 13,	17 "	22 "	18 "	18 "
" 16,	17 "	17 "	18 "	16 "
" 17,	18 "	16 "	18 "	15 "
" 18,	15 "	18 "	13 "	10 "
Av. Sensit. 1st week,	18 "	19.5 "	21.5 "	16.6 "
October 20,	22 mm.	18 mm.	10 mm.	15 mm.
" 22,	15 "	12 "	16 "	13 "
" 23,	11 "	15 "	12 "	10 "
" 24,	12 "	12 "	10 "	13 "
" 25,	8 "	7 "	7 "	7 "
" 26,	10 "	11 "	5 "	5 "
Av. Sensit. 2d week,	13 "	12.5 "	10 "	10.5 "

October 27,	10	mm.	8	mm.	9	mm.	8	mm.
" 29,	5	"	8	"	7	"	7	"
" 30,	4	"	6	"	4	"	9	"
" 31,	4	"	8	"	4	"	4	"
November 1,	5	"	2	"	4	"	5	"
" 2,	5	"	4	"	5	"	4	"
Av. Sensit. 3d week,	5.5	"	6	"	5.5	"	6.1	"
November 3,	5	mm.	6	mm.	3	mm.	3	mm.
" 4,	5	"	5	"	3	"	3	"
" 6,	4	"	4	"	2	"	3	"
" 7,	4	"	3	"	3	"	1	"
" 8,	2	"	4	"	2	"	2	"
" 9,	5	"	3	"	4	"	2	"
Av. Sensit. 4th week,	4.1	"	4.1	"	2.8	"	2.3	"
Gen. Av. Sensitiveness,	10.1	mm.	10.5	mm.	10	mm.	9	mm.

It was thought at first that there would be developed quite a difference in sensitiveness in both subjects between the morning and the evening sittings, and it can be seen by reference to the table that there is considerable difference in the averages for the first week, C. W. D. showing a greater sensitiveness during the morning sittings, while the opposite was true for F. B. D. It is interesting to note that this variation quickly decreased with practice and that after the first week there is practically no difference, thus following what might be termed a general law in education, viz.: one of the first effects of practice is to eliminate accidental hindrances, to render the work more and more mechanical and so less subject to variations due to unconscious influences. By referring to the general average, it is seen that the completed results for the month retain this preference in a slight degree, but it must be understood that these differences are due to those that arose in the first week.

Perhaps it would be well to call attention here to the absolute difference in sensitiveness, as shown by the table, for the two subjects. A great deal has been said about the comparative sensitiveness of the sexes with regard to touch, though a very few of such statements have been based on the results of accurate experiments. It would be wholly unscientific for me to make any general statement in regard to this question, simply because my experiments have been limited to two subjects. However, for these two the following may be said: In the first place it must be remembered that the part of the arm chosen for C. W. D. (a woman) was absolutely more sensitive than the part of the arm chosen for F. B. D. (a man), because that of the former was nearer the wrist than that of the latter. Hence the difference in sensitiveness at the start betwixt the two subjects should not be construed to mean that one or the

other had the greater power of discrimination, but that the difference arose chiefly because of the different positions of the parts exercised in the two subjects. By keeping this difference in mind, it is clear that F. B. D. showed in a slight degree the greater sensitiveness of the two. That is, he began with a part of the skin of less normal discriminative ability and succeeded in reaching a slightly higher discriminative ability than C. W. D.

Bilateral Transference of the Effects of Education of the Skin. One of the chief purposes of this work, as has been said, was to test the transference of the effects of exercise of the skin on one arm to that of the corresponding part of the other. These tests were made, as exhibited in the table after a month's work, and after it seemed that the approximate limits had been reached with regard to discriminative ability. The method used in making the tests was as follows:

The æsthesiometer was set for first test at 5 mm. whenever both points were pressed against the skin, in order to test what was considered the possible limit for discrimination before any exercise in testing might render the part more sensitive. Out of forty-five observations C. W. D. made but eleven errors, thus showing clearly that even when the points were so near, they could be recognized as two in seventy-five per cent. of the cases The next day another series of forty-five observations was taken with the points separated by 10 mm., when it was shown that eighty per cent. of them could be judged correctly. The first test made for F. B. D. was likewise for 5 mm., and out of fifty observations eighty per cent. of them were correctly judged. In order to show more clearly the transference of the increased ability to the corresponding part of the opposite arms, the following table is introduced, which shows the ability of both arms before and after the month's exercise.

BEFORE EDUCATING.

C. W. D.				F. B. D.			
Date.	Time.	Left Arm.	Right Arm.	Date.	Time.	Left Arm.	Right Arm.
Oct. 10.	8 A. M.	21 mm.	21 mm.	Oct. 10.	8 A. M.	33 mm.	33 mm.

AFTER EDUCATING.

Date.	Time.	Left Arm.	Right Arm.	Date.	Time.	Left Arm.	Right Arm.
Nov. 10.	8 P. M.	5 mm.	5 mm.	Nov. 11.	8 A. M.	2 mm.	5 mm.

From the result of these tests it is plain that either the education had crossed over, so to speak, or that the general discriminative ability had been wonderfully sharpened. That it was not largely due to the latter was proved in the follow-

ing way: Tests were made on the neighboring parts of the skin to those educated at the end of the month's work and the discriminative ability found to be much inferior to that of the part exercised and its corresponding part on the other arm. It is no doubt true that the general sensibility was slightly increased, but just how much, it is somewhat difficult to say. Unfortunately no general tests were made before the experiments began, to test whether or not the sensibility had been raised for distant parts of the body, such as one might naturally expect from education of the attention for this special kind of discrimination.

Other evidence, however, unexpectedly came out during the course of the experiments to prove that the education was limited to the locality in question. As was stated above tests were made on the territory surrounding the exercised part and its corresponding part on the other arm, which not only revealed the fact there mentioned, but also a marked difference in the *quale* of the local sensations; more striking in the arms exercised, but distinguishable in the others also. So much so that the subject could locate with a fair degree of accuracy the boundary of the exercised part merely through this acquired quality in the local sign. This difference in quality is best illustrated by the difference in the quality of the sensations, easily recognized, which arise from touching a finger-tip and immediately afterward some point on the back of the hand. There is not only an immediate recognition of a difference in location, but there is a different feeling present in the two cases. By touching the tips of two neighboring fingers, or for that matter one on each hand, the difference in the quality of the *feeling* is much less marked than in the former case. The change in the quality of the sensation, as Wundt [1] remarks, "is gradual as we pass from one point to another on the skin," and especially on the less discriminative parts of the skin is the change slow. Thus it was that this difference in the quality of the sensations was especially noticeable on the arms, for the change was so marked and so rapid near the boundary of the territory exercised and its corresponding part on the other arm that it attracted the attention of both subjects.

Given this change in the quality of the sensations, the question naturally comes, how did it arise? Surely not in a change in the fundamental structure of the end organ, because the time required for the development of this qualitative difference was too short for such changes. It must be that this rapid change was due to functional changes in the end

[1] Vorlesungen über Menschen und Thierseele. Vol. I. p. 214.

organ. The greatly increased exercise of the part would naturally tend to a larger development of these organs and hence there would come a quicker response to the stimulus, and especially a greater amount of nervous force would be liberated by a given stimulus. Wundt[1] says "the difference in the structure of the sense organ is the chief cause of the different quality of the sensation. Just as differences in tone and color fundamentally depend on the structure of the end organ in the ear and in the eye, so all qualitative differences which belong to any single organ have their necessary basis in the smaller variations, which may appear in the structure, or in the arrangement of the end organ."

The Rate of the Loss of this Increased Sensibility. Another purpose of this research, as stated above, was to determine as nearly as possible the rate of the loss of this increased sensibility, after the special exercise had ceased. It is clear that an exact curve of this kind can never be found, simply because the tests made to determine the exact state of the sensibility would naturally prevent the loss of the increased power as rapidly as it would occur without the tests. While, in a general way, it was known that the loss was rapid, no definite observations had previously been made, so far as I knew, on this special point. In determining how often the tests should be made in order to approximate the normal curve of such a relapse, there was, therefore, no help to be derived from well defined facts bearing on the question. It was decided, however, that the first test should be made one week after the special exercise had ceased, and let the results of this test aid in directing further experiments. It will be seen by the accompanying table that it was a mistake to wait so long before making the first test, for the decline at the first was very rapid, and decreased in rapidity inversely as the time. Later, tests were made more often, but varied in time, if, perchance thereby, a few points in the curve could be hit upon. But, as will be seen by the table given, the decline of this acquired sensibility was much more rapid than had been anticipated, and hence no definite curve can be given. The results here tabulated, however, give something of a glimpse of the true curve and in two places show that it was nearly reached. This table is of value chiefly as a guide to other workers in this line, though it is not without some value as an approximation to the facts sought.

[1] Op. Cit., p. 217.

TABLE SHOWING THE DECLINE OF THIS ACQUIRED SENSIBILITY.

For F. B. D.

DATE.	SPACE BETWEEN POINTS.	JUDGMENTS. Right.	Wrong.
Nov. 10.	5 mm.	40	10
" 11.	10 "	42	8
" 19.	5 "	25	25
" 22.	10 "	35	15
" 26.	15 "	32	18
Dec. 4.	20 "	32	18

Explanation of Table. On November 11 the tests showed that when the points of the æsthesiometer were separated by a space of 5 mm., eighty per cent. of the judgments were correct; on the same day in the evening with the points at 10 mm. apart, eighty-four per cent. of the judgments were correct. On November 19, with no intervening exercise, it was wholly impossible to distinguish with the æsthesiometer set at 5 mm., and three days later less than seventy-five per cent. of the judgments were correct at 10 mm., and so on for the other days, as indicated.

Observations. Throughout the entire series of experiments, note was taken of all subjective experiences which it was thought might elucidate, or direct in the experiments. The following statements summarize the chief points noted:

1. Some time must elapse between two successive applications of the æsthesiometer, so as to give time for the after-images to die away, otherwise these will fuse with the primary image and render differentiation much more difficult.

2. The discriminative ability is less acute at the beginning and close of the sitting than toward the middle. After a few experiments at the beginning, the sensibility increases with an increased flow of blood to the parts, while toward the close there is a slight dulling caused by fatigue.

3. Under certain conditions, not yet fully determined, much greater pressure was required to render the sensations clear and definite.

4. The same amount of pressure on different parts of the arm gives very different degrees of pain. That is to say, the normal amount of pressure on a certain part giving no pain, would, if exerted on another place, produce a sharp pain. It seems that this difference is not wholly due to difference in the thickness of the skin, but to inherent difference in sensibility to pain.

5. The discriminative ability is finer when the two points of the æsthesiometer are pressed against the arm in a cross direction than when they are pressed lengthwise. (Kotten-

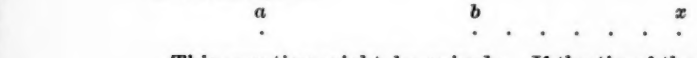
kamp and Ullrich found that this preference for the cross direction, for the upper extremities, over the lengthwise direction, was on the flexor side one-eighth, and on the extensor side one-fourth.)

6. Care was required during each sitting to keep the arm in a natural position, else the stretching of the skin rendered the sensibility abnormally acute.

III. Experiments on Open and Filled Space for Touch.

Tests with Active Touch. It is a well known fact that if two equal adjacent spaces are compared by the eye, the one filled and the other empty, the filled space will appear the greater. The purpose of the following series of experiments was to determine whether this would hold good for the sense of touch. That is, whether, of two equal spaces, the one offering the greater number of stimulations to the touch organs, as the finger is moved along through the spaces, would be judged the greater or not.

It is, perhaps, well to explain a little further here what is meant by open and filled space for touch. Of course, in a strict sense, there can be no empty or open space for touch. But by open space in this connection is meant a smooth, homogeneous surface, such as the surface of a bit of well-sized cardboard. By filled space we mean the same sort of a surface punctured from the under side, so as to offer to the sense of touch a series of additional and sharply defined sensations. The question arises here, how should these spaces be situated with regard to each other? Is there any source of error in the method adopted from Professor James, of dividing a line into two parts, marking a puncture at each extremity, and filling one part of this line with punctures and leaving the remainder for the open space, and thus be able to pass the finger over both spaces with one continuous movement? To illustrate, let the space between the dots at *a* and *b* in the accompanying diagram represent the open space, and that included between the dots at *b* and *x* represent the filled space:



This question might be raised: If the tip of the finger be passed from *a* to *x* and the subject be asked to judge of the comparative lengths of the spaces *ab* and *bx*, is it not to be expected from a physiological reason that *ab* would seem shorter, because its length would be judged on the basis of the sensations received from the time the *last* sensitive point of the finger tip left the puncture at *a* until the *first* one

touched at *b*? We should answer yes, and were it not that exactly the same thing occurs in case of the filled space, there would be a permanent source of error in the method, even for relative judgments. That these relative judgments have not been influenced in this way is plainly evidenced by the fact that the apparent length of the filled spaces depends on the *number* of punctures. Then, too, it might be supposed that retardation of the movement due to friction as the finger-tip passed over the roughened filled space would have a tendency to make the filled space seem longer, but that this was not the case is the testimony of all the subjects, because the filled spaces were not so rough as to prevent free movement.

This, too, is proved by the fact that increase in the number of punctures, although increasing the apparent length of the space in most instances, did not increase the roughness of the space, but on the contrary made it more easy for the finger to pass over. With these apparent objections out of the way, let us pass directly to the problem.

Prof. James[1] says, "If one divide a line on paper into equal halves, puncture the extremities and make punctures all along one of the halves; then, with the finger-tip on the opposite side of the paper, follow the line of punctures, the empty half will seem much longer than the punctured half." He does not say how often this was tried, or whether the results obtained were the same, when the finger passed first over the punctured half and then over the open, as when it passed in the opposite order. Neither is there anything said of the absolute lengths of the spaces compared. Thinking that these conditions, if they were introduced into the experiment, might influence the results, I have carried on a series of experiments in the following way to determine the influence of these conditions:

Apparatus. (1) Eighteen cards were made according to Prof. James' method from well-sized stiff card-board and about the same width, with the spaces to be judged similarly placed for each card. (2) The spaces to be judged ranged from 2 cm. to 16 cm., as can be seen in the accompanying table. (3) A number of cards were varied only in the number of punctures in the same space. That is, the punctures in these cards were made closer together. The punctures were all made with the same point, and, on any given card, in the punctured space were equally separated from each other. It seems probable, at any rate, that whatever the results might be, the introduction of these variables would be

[1] Psychology, Vol. II. p. 241. (Loeb found the opposite to be true for rough and smooth threads. Pflüger's Archiv, Vol. XLI. p. 121.)

helpful in arriving at some explanation. (4) Almost an equal number of experiments were taken with each of the spaces presented first, for it was thought perhaps that the after-images of the punctures when presented first might appear to lengthen the punctured part.

The first series, that of C. W. D., was made on a wholly naïve subject, and at no time were the cards permitted to be seen or the purpose of the experiment mentioned. The cards were never presented in the same way two successive times, and the short ones were distributed so that no possible means of establishing an incidental and illegitimate means of judging was brought in. The other series given in the table were made on men who had thought and read more or less on the point, and could not be said to be ignorant of the purpose of the experiment. Their records, however, show that they judged according to their sensations, and hence they are in the main uniform.

TABLE II. Part 1.

COMPARISON OF OPEN AND FILLED SPACE THROUGH SENSE OF TOUCH.

		a. Open Presented First.						*b.* Filled Presented First.						Totals.	
		4	9	4	7	13	9	9	11	6			
		2-2	2-2	3½-3	3½-3	3½-3	4-4	4½-4	4½-5	5-5			
		Open	Filled	Open	Filled	Open	Filled	Open	Filled	Open	Filled	Open	Filled
C. W. D.	*a*	1	18	0	19	7	10	3	17	3	15	3	16	2	17	0	15	3	17	22	144
	b	1	18	0	17	4	14	4	15	2	16	1	13	3	13	0	12	4	12	19	130
J. A. B.	*a*	0	2	0	3	0	2	0	3	0	3	0	3	0	3	0	3	0	1	0	23
	b	0	2	0	2	2	0	1	1	0	2	0	0	0	1	1	1	1	0	5	9
J. H. L.	*a*	0	3	0	3	0	3	0	3	0	3	0	3	0	2	0	3	0	3	0	26
	b	0	3	0	3	2	0	1	2	0	3	1	1	0	3	2	1	0	2	6	18
J. S. L.	*a*	1	2	0	4	2	3	2	2	2	2	1	3	1	4	0	3	2	0	11	21
	b	0	2	0	2	1	1	0	2	0	2	0	2	0	2	0	2	0	1	1	16
J. A. H.	*a*	0	3	0	3	0	3	0	3	0	3	0	2	0	3	0	3	0	3	0	26
	b	0	2	0	2	1	1	0	2	1	1	0	2	1	1	0	2	1	1	4	14
H. A. A.	*a*	1	1	0	2	3	0	4	0	1	3	0	3	1	3	2	2	2	3	14	17
	b	0	2	0	2	0	2	0	3	0	1	1	0	1	2	0	2	2	1	4	15
F. D.	*a*	2	1	1	1	2	0	2	2	2	2	1	3	0	4	1	3	2	0	13	16
	b	0	4	0	2	1	3	1	4	0	4	0	0	1	4	3	1	4	0	10	22
Totals.		6	63	1	65	25	42	18	59	11	60	8	51	10	62	9	53	21	44		

Explanation of Table. Parts 1 and 2. The numbers 4, 9, 4, 7, etc., at the head and on the right of the double columns, indicate the number of punctures made in the number of cm. denoted by the number immediately below in the same column. Thus, in the first double column, the figure *4* at the head and on the right indicates that in the space of 2 cm. on the card there were four punctures; in the second double column the figure *9* indicates that in the number of cm. denoted

TABLE II. Part 2.

COMPARISON OF OPEN AND FILLED SPACE THROUGH SENSE OF TOUCH.

a. Open Presented First. *b.* Filled Presented First.

		11	13	7	10	19	12	13	14	16	TOTALS.	
		5–5	5–6	7–6	11–9	11–9	9–11	12–12	14–13	16–15	Open	Filled
C. W. D.	*a*	1 11	0 16	6 10	12 1	9 6	7 8	6 5	8 4	17 11	66	62
	b	4 8	2 12	3 11	8 2	7 7	6 7	9 2	7 6	21 2	67	57
J. A. B.	*a*	0 3	0 2	1 2	3 0	1 2	0 2	0 2	0 3	1 2	6	18
	b	1 0	0 2	2 0	2 0	1 0	1 0	0 0	0 1	1 0	8	3
J. H. L.	*a*	0 3	0 3	0 3	0 3	1 2	0 3	0 3	0 3	0 3	1	27
	b	1 1	1 3	3 0	3 0	3 0	3 1	2 0	3 0	3 0	22	5
J. S. L.	*a*	0 2	1 3	4 0	3 0	5 2	1 3	3 1	2 2	3 1	23	14
	b	0 2	0 2	1 1	2 0	2 0	0 2	1 1	1 1	1 1	8	10
J. A. H.	*a*	0 3	1 2	1 2	1 2	1 2	0 3	0 1	0 3	0 2	4	20
	b	1 0	0 2	2 0	2 0	2 0	2 0	2 0	2 0	2 0	15	4
H. A. A.	*a*	0 2	0 4	2 1	2 1	2 1	1 3	1 3	0 3	0 3	8	21
	b	0 1	0 1	2 0	2 0	2 0	2 0	2 0	2 0	2 0	14	2
F. D.	*a*	1 0	1 3	4 1	4 2	3 3	1 4	0 2	1 3	0 3	15	21
	b	1 0	3 1	4 0	4 0	4 1	4 0	2 0	3 1	2 2	31	5
Totals.		10 36	10 56	35 31	48 11	43 26	28 36	29 20	29 30	53 30		

by the figure immediatly below there were nine punctures, etc. Thus, in the first two double columns, the cards were exactly alike save in the number of punctures in the filled side, the one having but four, the other nine, punctures in 2 cm.

Results: With C. W. D., out of nineteen judgments between an open space of 2 cm. and the same amount of adjacent space punctured with four holes equally separated, eighteen judgments were made in favor of the punctured space being longer, to one in favor of the open space being longer when the open was presented first, and exactly the same proportion when the punctured was presented first. In the second column the same length of spaces was used (that is, 2 cm. for the open and the same for closed), but instead of only four punctures there were nine in the filled space. Here it will be seen that the filled space was judged to be longer every time for C. W. D., and by referring to the accompanying table, it will be seen that out of a total of sixty-six judgments for this card sixty-five of them were in favor of the filled space being longer. In the next three columns, that is, in the third, fourth and fifth, are tabulated the judgments rendered on three cards, all of which were exactly alike save in the number of punctures in the filled space. It will be seen, by referring to the totals in these columns, that increase in the number of punctures in the same space increased the number of judgments in favor of the filled space being the longer. Thus C. W. D., in the

first column of this set, made seventeen judgments; ten were in favor of the filled space being longer, and seven in favor of the open. In the second column of the sets twenty judgments were made, seventeen in favor of the filled space and three in favor of the open. In the third column of the set eighteen judgments were made, with fifteen for the filled and three for the open. By glancing at the totals at the bottom of these columns, it will be seen that these proportions were more than borne out by the other subjects. That is, the totals show that with the increase in the number of punctures in the same space, the space appeared longer even in the third column of the set. These judgments, it must be remembered, were between two spaces: $3\frac{1}{2}$ cm. of open space and 3 cm. of filled space; so that, even if the number of judgments were equal for the two, it would show a preference for the filled space. But when it is seen that under these conditions, the totals show the ratios of preference to be $1\frac{2}{7}$, $3\frac{1}{3}$ and $5\frac{1}{2}$ for the filled spaces respectively, it makes a clear demonstration, not only that filled space seems longer for active touch, but that the greater the number of separate sensations received in the filled space, the longer it appears to be. Now, it is clear that this increase in the apparent length of the filled space would not indefinitely increase with the increase in the number of punctures, but that there would be a point reached where these would not be separately distinguishable and the number of the sensations would diminish rather than increase, so that we should have a right to expect a decrease in the number of judgments preferring the filled rather than the open as the longer.

This same influence of the number of punctures, or in other words, the number of distinct sensations received in passing over the same amount of space, is seen by comparing the last double column of judgments in Part 1 with the first in Part 2. Here the spaces in each case were the same; that is, 5 cm., but in the former there were but six punctures in the filled space, while in the latter there were eleven. By glancing down these columns, it is plain to see that the filled space in each case was preferred, but much more often relatively in case of the card with eleven punctures. The totals show the ratios of the judgments to be two and one-tenth and three and two-fifths respectively. That is to say, there were two and one-tenth judgments, in the first case, in favor of the filled to one in favor of the open, but in the second there were three and two-fifths to one in favor of the filled. It seems that no further explanation of the results bearing on this point need be made, but that this is seen to be true, within certain limits, in all parts of the table. However, it is clear that as the

spaces to be compared increase in length, the influence of the filled space is less and less marked. That is to say, when the spaces to be compared are more than 10 cm. in length, the illusion does not hold so steadily. For example, the open space is judged longer more times than the filled where each space was 12 cm., although the filled had thirteen punctures. It is evident that, with the longer spaces, the subjects were guessing quite largely, though in a general way the filled spaces were preferred for the longer. The notes I made at the time of making the experiments are valuable here. With no exceptions, the subjects complained of the length of these longest cards, constantly saying they had no sure basis for their judgments, often saying that the judgment was a mere guess. Especially, they complained of the long open space as being so indefinite, because, in passing the finger over it, there was a sort of anxious effort to guide the finger through this blank, so to speak, in order that it would strike clearly the first puncture of the filled space. The hesitancy and caution here introduced would have a tendency to make these longer open spaces appear longer than they otherwise would, and in this manner in a slight degree, at least, vitiate the results. However, as has been said, even, in a general way, the longest open spaces used do not seem so long as equally long filled spaces.

The next question is, what difference in the judgments arose when the open space was presented first as compared with those where the filled came first? Or, in other words, when two equal spaces are to be compared as to their length by touch, one filled and the other open, does it make any difference in the judgments which is given first? This question can be answered by saying that when the spaces to be compared are short, less than 5 cm. each, there is practically no difference, but that above this, as far as we went, there is quite a difference, though varying slightly with different subjects. By examining the double columns of totals on the right of each of the foregoing tables, these statements are made more clear. Take, for example, the first subject again, C. W. D., it will be seen that in Part 1 the whole number of judgments rendered in favor of the open being the longer was twenty-two when the open was presented first, and nineteen when the filled was presented first, and 144 and 130 respectively in favor of the filled as being the longer. Of course, twenty-two and nineteen can not be directly compared unless the whole number of judgments in the two cases were the same. As this was not the case, the ratios of the judgments in the two cases should be compared and these are found to be the same, viz.: one to four and five-tenths in

each case. By following down the columns further in the same part, it will be seen that for some subjects there seems to be quite a difference, even when the spaces were less than 5 cm., but as there were comparatively few experiments made on each of these subjects, the difference ought not to count for too much. But in Part 2, it will be seen that for every subject save one, when the filled space was presented first, there was an increase in the number of judgments preferring the open spaces as the longer. Just why this should be so is not so easy to say. But it is perhaps due to the rapidity with which the sensations die out as the finger is moved through the spaces. Here again, the notes made during the progress of the experiments are helpful in making an explanation. It was observed that the longer the spaces compared, the greater the number of trials that were required for the subjects to come to a decision. Not only this, but each of them remarked that the long ones were very indefinite, and that by the time the finger was passed over both spaces, no definitely clear notion of the length of the first one was left.

Tests with Passive Touch. The problem of getting regular and steady motion of the proper rate for passive touch occupied much of my time before it was finally and satisfactorily solved. The conditions which it was necessary to satisfy were: (1) That the motion should be of uniform rate; (2) that it should be unaffected by any slight friction, and (3) that the card should thus be brought in contact with the finger with sufficient and regular frequency. Happening to be in the shop of the university mechanic one day, I noticed that in the movement of a planer for metals, which was run by a steam-engine, the above conditions could be easily fulfilled. Its movement was horizontal and could be regulated as to speed and frequency of return, while at the same time the apparatus was so massive that the motion would not in the least be influenced by a small amount of friction. All that was required, then, in order to make this serve the purpose, was to arrange a stationary arm rest, and to fasten a rack to the moving bed of the planer to hold the cards. Having made these arrangements, the conditions were then fully satisfied and the same tests could be made with the cards for passive touch that had been made for active touch.

The subjects were seated at right angles to the moving bed of the planer with the arm at rest. By simply extending the finger tip over the end of the arm rest, it was thus brought into contact with the punctures of the moving card. In this case, as in the tests with active touch, the finger-tip was kept in contact with the card only when the motion was in one

direction. To do this, the finger was raised from the card during the backward motion of the planer. The subject thus had opportunity to try as often as he wished before coming to a decision as to which of the two spaces was the longer.

In all the tests with passive touch, the cards were presented with the open space always coming first. It was deemed unnecessary to take any tests with the spaces reversed, because of the negative results with this variation in the tests with active touch. The subjects were blindfolded during the tests and directed to make their judgments wholly upon the basis of their sensations.

Under these conditions, as stated, it is very clear that all aid in determining the relative lengths of the two spaces, coming from the sensations derived from voluntary movement, was eliminated. That is to say, the various judgments of the subjects were based only on the sensations of touch. Cards with punctures, as previously described, were used, because with these there was much less likelihood of introducing an element of pain. As soon as the points became dulled by repeated touchings, new cards were substituted, which thus kept the spaces constant and their relations unchanged.

The following table shows the judgments of the subjects there named, under the above conditions:

	2½-2		2-2½		2½-2		2-2½		2½-2		2-2½		3-2	
Y.	0	8	0	8	4	4	0	8	6	2	0	8	5	3
H.	0	3	0	3	0	3	0	3	0	3	0	3	1	2
T.	0	3	0	3	0	3	0	3	0	3	0	3	0	3
J.	0	5	0	5	0	4	0	4	2	2	0	5	0	5
Others.	0	2	0	2	0	2	0	2	0	2	0	2	0	2
Totals.	0	21	0	21	4	16	0	20	8	12	0	21	6	15

	3½-3		3-3½		4-3		4-2½		2½-4		4-3½		3½-4	
Y.	0	9	0	9	3	6	4	5	0	9	2	7	0	9
H.	0	3	0	3	1	2	0	3	0	3	0	3	0	3
T.	0	3	0	3	0	3	0	3	0	3	0	3	0	3
J.	1	4	0	5	3	2	5	0	0	5	2	3	1	4
Others.	0	4	0	4	0	4	1	3	0	4	0	4	1	3
Totals.	1	23	0	24	7	17	10	14	0	24	4	20	2	22

Explanation of Table. This table is identical in plan with the one given for active touch, save in this one there is no specification of the number of punctures in each filled space. The number was not indicated, because the punctures were made equally far apart in each case.

Results. There were 312 judgments under these conditions, of which 270 were in favor of the filled space. This too, in spite of the fact that the open spaces of the fourteen cards used were really longer than the filled. If we consider the

judgments made, when the open spaces were always longer than the filled spaces with which they were compared, we will still find a great preference for the filled space. The number of judgments made from a comparison of the spaces on the eight cards whose open spaces are always longer than their filled spaces, is 178. Of this number the filled spaces were judged to be the longer 138 times, while the various open spaces were judged to be the longer only forty times. It is a waste of space and time to take up each card and show that in every case the totals give a greater number of judgments for the filled space. If the reader will only glance at the results of each column this fact will be manifest.

Tests when the Tactile Surface of the Finger-tip was Limited. It was mentioned previously that there might seem to be a fault in the method thus far used, in that when the entire width of the tip of the finger was brought into contact with the points, the notion of the length of the open space would be formed from the space passed over from the time the *last* sensitive point of the finger left the point at the beginning of the open space, until the *first* sensitive point touched the beginning of the closed space; thus, of course, making the open space appear shorter than it really was. While, as was said, it was thought that this was counterbalanced by the same thing happening with the closed space, to prevent any possible error in the results, the following variation in the method for passive touch was introduced. It is plain that if there be any fault in the method as indicated, it could only be remedied by limiting the amount of the tactile surface of the finger-tip to the space of a single sensory circle. This was accomplished in the following way: The arm-rest previously mentioned was lengthened so as to pass entirely across and above the moving card-rack; directly over this moving rack, a slot was cut in the board forming the arm-rest and into this slot a strip of tin was fitted so as to extend downward below the lower side of the board and approach the surface of the card. Through this tin, a hole was made just so large that when the finger was pressed against it, a portion of its sensitive surface, less than 2 mm. in diameter, protruded and came into contact with the punctures of the card passing along underneath. By this means, not only was the possible error in the results avoided, but all motion of the finger, either real or suggested, was wholly eliminated. All the conditions of this series of tests were the same as those in the previous tests, save the variations above described. It perhaps might be well to say here that punctured cards were used throughout the whole series of tests.

The tests made under these last conditions give the following condensed results: By using cards made and spaced exactly as those used in the tests with passive touch when the finger-tip was not limited, out of 200 judgments, only thirty-three were in favor of the open, while 167 were in favor of the closed being the longer, thus maintaining practically the same proportion of preference for the closed as in the tests with the other methods. These judgments were purposely obtained from subjects who had not served for me in the previous tests, in order to be sure that they be completely naïve.

The observation made by Herr Mellinghoff, as reported by Wundt,[1] to the effect that if the space between two points be divided into two equal parts by a dot, and then one of these parts be again divided into halves by another dot, the half of the whole space thus filled seems shorter for the eye than the open half, does not hold true for touch. To see whether or not this held for touch, I made a series of experiments with cards prepared to meet the above conditions, but found in the tests made thereon that the preference was as marked for the filled space under these conditions as it was in any of the previous tests for passive touch.

The question now arises, what is the meaning, from a psychological point of view, of these results? In the first place, it is plain that a closed space for the sense of touch, just as for that of sight, appears longer than an equal open space. Hence the explanations heretofore given, wholly based on the difference in time the eye used in passing over opened and filled space, must stop short of the real truth. That this illusion holds for touch is, I think, completely demonstrated by my experiments in passive touch, and in these the time used in passing over equal spaces was exactly the same. Hence, differences in this element cannot here enter into the explanation. The fact is, that in the comparison of these two separate trains of sense-perceptions, the one which was the fuller of definite and distinct sensations seemed the longer, though equal time had been consumed in the passage of each train across the focus of consciousness. All have noticed how much longer a road seems when first passed over, than it does after it becomes a familiar and often traveled road. The first time it is passed over, everything appears in consciousness, even to the minor details of the scenery, but as it becomes more familiar, there come to be only a few points, such as half-way places, or some peculiar objects, which are noticed at all, while the

[1] *Physiologische Psychologie*, 4th ed. Vol. II. p. 142.

stretches between are as open spaces. One cannot read Stanley's[1] description of the great African forest without feeling the vastness of it, chiefly because he makes one crowd into a small space of time, images of multitudes of plants and shrubs and trees and insects, and then reminds you that all you have in mind must represent only those things seen within a small radius, for you cannot even see through to the sunlight. Besides, he merely mentions and hastens, leaving the reader with only suggestions from which to bedeck the trees with their varied foliage and festoon them with myriad vines.

In the tests I made with the cards, it was not only found that of two equal spaces, the one open and the other filled, the filled one seemed the longer; but, also, it was found that of two filled spaces of equal extent, the one more completely filled seemed the longer. It must be remembered, however, that "more completely filled space" in this connection means capacity to present to the subject in a given time a greater number of distinct sensuous stimuli. It is clear that if too great a number of distinct stimuli be poured in upon the subject in a given time, they would drop below the threshold of differentiation, and such a train would be classed with those which arise from what we have termed open space. For example, if we run the fingers rapidly over a piece of coarse cloth or a wire screen, instead of a number of distinct sensations, there is but one continuous undifferentiated sensation; or if we glance over the lawn, the sensation is as if there were but one continuous stimulus, though we know it to be produced by the fusion of the stimuli from countless grass-blades.

The conclusion to which our results lead us, then, is that if we have given two trains of sense-perception produced in the mind in equal durations of time, the one which is made up of the greater number of distinct sensations requires of the mind a greater amount of space in its reference to the external world, provided, of course, that these trains are special at all.

It should be noted that this generalization says nothing of the origin of our notion of space, but only calls attention to the fact as exhibited in adult mental life. As to whether this be an inherent mental tendency or the result of habitual associations, the tests here made have nothing at all to say. There is thus offered abundant opportunity for speculation and hypotheses, but we prefer to run no risk in parting company with the truth and so will be content to await the outcome of a continuation of these investigations.

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[1] In Darkest Africa, Vol. II. p. 72.

IV. Illusion for Weights. A Study in Association and Apperception.

It has long been known that of two bodies of equal weight, but of different size, the smaller one appeared the heavier when lifted. The purpose of the following series of experiments was to determine how the perception for certain weights could be influenced when (1) they were of the same *shape* and *weight*, but different in *size*, and (2) when they were of the same *size* and *weight*, but different in *shape*. The well-known experiments of E. H. Weber[1] and Fechner[2] will be recalled here. They experimented with weights of the same *shape* and *size*, but of different absolute *weight*. The purpose of these workers was to determine the fineness of discrimination for difference in weight. The work here described is of a very different nature from theirs, nevertheless the results obtained show the need of great care in the preparation of weights for such work as theirs and in so far is supplemental to their work. The purpose of this work, however, was to make a study in association and apperception by making use of one of the strongest associations we make in life, viz., that of two given weights of the same material the larger is the heavier. This association we have firmly fixed because of the often recurring and immediate sequence of the ideas. Indeed, if it could be said of any association, that it is inborn or racially fixed, it could be truly said of this one.

Tests on School Children.[3] Upon these a study was made concerning the illusions arising from comparing weights of the same shape and weight, but different in size. To make this study, I constructed some weights in the following way: A brass tube, a little over an inch in diameter, was cut into small tubes of the following lengths: No. 1, 1½ in.; 2, 2 in.; 3, 2½ in.; 4, 3 in.; 5, 3½ in.; 6, 4 in.; 7, 4½ in.; 8, 5 in. They were each made to weigh 132 grams by filling them with different substances and were all capped over with carefully fitted bits of brass. Care was taken to distribute the weight throughout the whole of each tube, so that one end

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[1] Der Tastsinn und das Gemeingefühl, Wagner's Handwörterbuch der Physiologie, 3, p. 543 et seq.

[2] Elemente der Psychophysik, I. p. 183 et seq.

[3] I am greatly indebted for the material here used to Mrs. Grace B. Sudborough, Principal of the Teachers' Training School, Omaha, Nebraska, who carried out under my direction this long series of tests on the public school children of Omaha. Her work of making the tests was of the most painstaking and careful kind and deserving of the highest commendation. Also, my thanks are due to Supt. Fitzpatrick, of the city schools of Omaha, for his interest and coöperation in the work.

should not be heavier than the other and thus introduce a disturbing element when they were lifted. There was nothing in the external appearance of any of them which would in any way suggest a difference in weight save their difference in size. It will be seen that the tubes increased in length regularly by half an inch and when set up they formed a series of regularly ascending steps. The design of this arrangement was to determine what effect such a relationship would have in suggestion so as to continue the illusion for the whole series. The suggestion thus made became so strong that, when the subjects began to see that the tubes could be arranged in a regular order, in many instances the judgments were changed and the tubes were made to occupy a place according to size. In some cases, however, this suggestion had the opposite effect; that is, it caused a disarrangement which would probably not otherwise have been made, simply, as the subjects afterward acknowledged, because they thought it improbable that the tubes had been made so as to decrease in weight so regularly. These, of course, were not strictly legitimate judgments, and should be cast out of the results wherever they are known to exist. But as there were very few of such they have not appreciably influenced the final results. In all cases where the subject permitted his judgments to be made from the sensations purely, the arrangement is probably without a variation, in order of their size. So with most of the subjects the illusion was strengthened by attracting their attention to this regular difference in size. Constant care was taken in making the experiments not to suggest to the subjects that they were given a puzzle to work, or that there was anything strange about the experiment. The tubes were put before them and they were simply asked to "arrange them in order of their weight." These words were used because it was thought they were freest from suggestion. The subjects thus began the experiment with the notion that it was a test in fineness of discrimination for weight. The tests were made under the most favorable circumstances possible. No one was permitted to be present but the operator while the tests were being made, in order to prevent, especially in the case of the younger children, any possible confusion or unnaturalness. They were directed to lift each weight in the same way, that is, by taking them between the thumb and finger and lifting them straight up, in order to prevent any physiological basis for a difference in sensations. No more restrictions were placed upon them, however, than were necessary to insure the introduction of no error. After the weights had been arranged in the order, as they thought, of their weight, they were

asked to make a comparison of the weights in relative terms, of the first and the last of the series. These comparisons have all been entered in the tables.

Before the work began, the children were divided into three classes according to their general intelligence. The brightest were grouped together to form class I, the good to form class II, and the dullest were put in class III. This classification was made by their teachers, and was unknown to the children. They were simply marked in the tables as belonging to a certain class. Also, their age, sex, nativity and physical development were noted, as well as the nativity and occupation of the parents. All this but the latter will be seen by a glance at any one of the tables given.

The following tables show the arrangements of the weights and their comparisons for the groupings with reference to grade of intelligence, sex and age.

CLASS I. BOYS.

Age.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	Comparative weight of first and last of series.
7	1	2	3	4	5	6	7	8	2
8	1	2	3	4	5	6	7	8	3
8	5	4	2	1	3	6	8	7	3
8	1	3	2	4	5	6	7	8	3
8	1	2	3	4	5	6	7	8	4
8	1	3	2	4	5	6	7	8	2
8	1	2	3	4	5	6	7	8	2
8	1	2	3	4	5	6	7	8	3
8	1	2	3	4	5	6	7	8	2
9	1	2	3	4	5	6	7	8	2
9	1	2	3	4	5	6	7	8	3
10	1	2	3	4	5	6	7	8	5
10	1	2	3	4	5	6	7	8	6
10	1	2	3	4	5	6	7	8	4
10	1	3	2	4	6	5	7	8	2
11	1	2	3	4	5	6	7	8	3
11	1	2	3	4	5	6	7	8	5
11	1	2	3	4	5	6	7	8	2½
12	1	2	3	4	5	6	7	8	2
13	3	2	1	4	5	6	7	8	3
14	1	2	3	4	5	6	7	8	1½
14	1	2	3	4	5	6	7	8	2

Class I. Girls.

Age.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	Comparative weight of first and last of series.
6	1	2	3	4	5	6	7	8	3
7	1	2	3	4	5	6	7	8	2
7	1	2	3	4	7	5	6	8	2
7	1	2	3	4	5	8	6	7	4
7	1	3	2	4	5	6	7	8	5
7	1	2	3	4	5	6	7	8	2
7	1	2	3	4	5	6	7	8	6
8	1	2	3	5	4	6	7	8	3
8	1	2	3	4	5	6	7	8	2
8	1	2	3	4	5	6	7	8	3
8	3	1	2	4	6	5	7	8	3
9	1	2	3	4	5	6	7	8	3
9	1	2	3	4	5	6	7	8	2
9	1	2	3	4	5	6	7	8	3
9	1	2	3	4	5	6	7	8	3
10	1	2	3	4	5	6	7	8	2
10	1	2	3	4	5	6	7	8	3
10	1	2	3	4	5	6	7	8	3
10	2	1	3	4	5	6	7	8	3
10	1	3	2	5	6	4	7	8	2
10	1	3	5	2	4	7	6	8	2
11	3	2	1	4	5	6	7	8	2
11	1	5	2	3	4	6	7	8	3
11	1	2	3	4	5	6	7	8	2
11	1	2	3	4	5	6	7	8	9
11	1	2	3	4	5	6	7	8	4
11	1	2	3	4	5	6	7	8	1
12	1	2	3	5	4	7	6	8	2
12	1	3	2	4	5	6	7	8	2
12	1	2	3	4	5	6	7	8	2

Class II. Boys.

Age.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	Comparative weight of first and last of series.
7	1	2	3	4	5	6	7	8	3
7	1	2	4	3	8	7	6	5	2
7	1	2	3	4	5	7	6	8	2
7	1	2	3	4	5	6	8	7	5
7	1	2	3	4	5	6	7	8	3
7	2	1	3	4	5	6	7	8	4
8	1	2	3	4	5	6	7	8	3
8	1	2	3	4	5	6	7	8	2
9	1	3	2	4	5	6	7	8	5
9	1	2	3	4	5	6	7	8	6
9	1	2	3	4	5	6	7	8	6
9	1	3	4	2	5	6	7	8	3
9	1	2	3	4	5	6	7	8	2
9	1	2	3	5	4	6	7	8	3
9	1	2	3	4	5	6	7	8	4
9	1	2	3	4	5	6	7	8	2
9	1	2	3	4	5	6	7	8	2
10	1	3	2	4	5	6	7	8	2
10	1	2	3	4	5	6	7	8	3

Class II. Boys.—*Continued.*

Age.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	Comparative weight of first and last of series.
10	2	1	3	5	7	6	8	4	1½
10	1	3	2	4	5	6	8	7	2
11	1	2	7	3	4	5	6	8	4
11	1	2	6	4	3	5	8	7	3
11	1	2	3	4	5	6	7	8	2½
11	1	2	5	3	4	6	7	8	3
11	1	2	3	4	5	6	7	8	3
12	4	1	3	2	5	6	7	8	1½
12	1	2	3	4	5	6	7	8	3
12	1	2	3	4	5	6	7	8	3
12	1	2	3	4	5	6	7	8	3
13	1	2	3	4	5	6	7	8	2
13	1	4	3	2	6	5	8	7	2
13	1	2	3	5	4	6	7	8	3
13	1	3	2	4	5	6	7	8	3
13	1	2	3	6	4	5	7	8	2
13	1	2	4	3	5	6	8	7	1½
14	1	2	3	4	5	6	7	8	3
14	1	2	3	4	5	6	7	8	8
14	1	2	3	4	5	6	7	8	4
14	1	2	3	4	5	7	6	8	2
14	1	4	3	2	6	5	8	7	3

Class II. Girls.

Age.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	Comparative weight of first and last of series.
7	3	1	2	4	5	6	7	8	0
7	2	3	1	4	5	6	7	8	2
7	1	2	3	4	5	6	7	8	3
8	1	2	3	4	5	6	7	8	2
8	1	2	3	4	5	6	7	8	4
8	1	2	3	4	5	6	7	8	3
8	3	1	2	5	4	6	7	8	2
8	1	2	3	4	5	6	7	8	3
9	1	2	3	4	5	6	7	8	8
9	1	3	2	4	6	5	7	8	3
9	1	2	3	4	5	6	8	7	3
9	1	2	3	4	6	5	7	8	2
10	1	3	2	4	8	6	5	7	2
11	1	2	3	4	5	6	7	8	3
11	1	2	3	5	4	6	7	8	3
11	1	2	3	4	5	6	7	8	4
11	1	2	3	4	5	6	7	8	5
12	1	2	3	4	5	6	8	7	2
12	1	2	3	5	6	4	7	8	2
12	1	2	3	4	5	6	7	8	6
13	1	2	4	3	6	5	8	7	2
13	3	1	5	2	4	7	6	8	3
13	1	2	3	4	5	6	7	8	4
13	1	2	3	4	5	7	6	8	3
13	1	2	3	4	5	6	7	8	2
14	1	2	3	4	5	6	7	8	3
14	1	2	3	4	5	6	7	8	3
14	1	2	3	4	5	6	7	8	6
14	1	2	3	4	5	6	7	8	8
14	2	1	4	3	6	5	7	8	3

Class III. Boys.

Age.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	Comparative weight of first and last of series.
7	1	2	3	4	5	6	8	7	2
7	1	2	5	3	4	6	7	8	2
7	1	2	3	4	5	6	7	8	3
8	1	2	4	3	5	6	7	8	3
8	1	2	3	4	5	6	7	8	4
9	1	2	3	4	5	6	7	8	4
10	1	2	4	3	5	6	7	8	2
10	2	1	5	3	4	6	7	8	2
10	1	3	2	5	4	6	7	8	7
11	1	2	3	4	5	6	7	8	2
11	2	1	3	5	4	6	8	7	2
11	1	2	3	4	5	6	7	8	2
12	2	1	3	4	6	5	7	8	3
12	1	2	3	4	5	6	7	8	2
12	1	2	3	4	5	6	7	8	3
12	1	2	3	4	5	6	7	8	2
12	4	2	5	3	1	6	7	8	2
13	1	2	3	4	5	6	7	8	2
13	2	1	3	4	5	7	6	8	4
13	1	2	3	4	5	6	7	8	5
13	1	2	3	4	5	6	7	8	3
13	1	2	3	4	5	6	7	8	5
13	1	3	2	4	5	6	7	8	1½
13	2	1	3	4	5	6	7	8	2
14	1	3	2	4	6	7	5	8	5
14	4	3	2	1	5	6	7	8	2
14	2	1	3	4	5	6	7	8	3
14	2	1	3	6	5	4	7	8	2

Class III. Girls.

Age.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	Comparative weight of first and last of series.
6	1	2	3	4	5	6	7	8	3
7	1	2	3	4	5	6	8	7	3
7	1	3	2	4	5	6	7	8	2
8	1	2	3	4	5	6	7	8	2
9	1	2	3	4	5	7	6	8	4
10	1	2	3	4	5	6	7	8	3
10	1	2	3	4	5	6	7	8	2
10	1	3	2	4	8	5	6	7	2
10	1	2	3	4	5	7	6	8	3
11	1	2	3	4	5	6	7	8	3
12	1	2	3	4	5	6	7	8	3
12*	3	7	4	2	1	5	6	8	3
12	1	2	4	3	5	6	7	8	2
12	1	3	4	2	5	6	7	8	2
12	1	5	3	6	2	4	7	8	3
13	2	1	5	3	4	6	7	8	3
13	1	2	3	4	5	6	7	8	3
14	1	2	3	4	5	6	7	8	3
14	2	4	1	3	7	6	5	8	2
14	2	1	3	4	5	6	7	8	2
14	3	1	2	4	5	6	7	8	5
16	1	2	3	4	5	6	7	8	2½
18	1	2	5	4	3	7	6	8	3

*Very dull.

Explanation of the Tables. The Roman numerals at the head of the columns represent the arrangement of the order of eight weights. All the weights which were judged to be the heaviest were recorded in the columns marked I; those weights which were judged to be the next lightest, in the columns marked II, and so on to those judged to be the lightest, which was thus recorded in column VIII. The Arabic numerals entered beneath in these columns represent the weights which were judged to belong in this order of weight. Thus, the figures 1, 2, 3, 4, 5, 6, 7, 8, represent the weights in order of their size: 1 represents the weight 1½ inches in length; 2 represents the one 2 inches; 3, the one 2½ inches, and so on up to 8, which represents the longest one, 5 inches in length. So, when the figures correspond with the Roman numerals at the head of the columns, it means that their weight was judged to be in the inverse order of their size. The smallest was judged to be the heaviest, and the largest the lightest.

In the columns marked "comparative weight of the first and last of the series," the figures are entered which represent the number of times the weight entered in column I was judged to be heavier than that entered in column VIII. The figures representing any disarrangements of the weights in order of their size are printed in bold-faced type, in order that they may appear more distinct in the tables.

The Arrangement of the Weights. A single glance at the tables is sufficient to see that more than half of the children arranged the weights in the *exact* order of their size. By actual count, it will be found that ninety-two out of 173 arranged them so, and that of all the others, not *one* reversed them, or even made an approach to a reversal, notwithstanding it was supposed that with the younger children this would be the case. Then, the degree of the disarrangement is of much importance. What is meant by degree of disarrangement in this connection can be best explained by taking a concrete example. In the table marked Boys, Class I, the third boy has made the arrangement, 5, 4, 2, 1, 3, 6, 8, 7; here, in this case, weight 5 was judged to be the heaviest, and so on in the order they occur. Now, it is plain that his arrangement is much more a departure from the normal than it would have been had he placed them so that the weights would in no case be arranged more than one place removed from the column in which it would be normally placed. Instead of weight 5 being put in either column IV or VI, which would be a displacement of the *first* degree, it is put in column I, which makes it a displacement of the *fourth*

degree from the normal. The degree of displacement, then, can always be found by finding the difference between the number of the weight and the number at the head of the column in which it is placed. It is easy to see that, of all the displacements made, by far the greatest number are only of the first degree. In fact, out of 287, the whole number of displacements, 218 were of the first order, fifty-one were of the second order, thirteen of the third order, fourteen of the fourth, and one of the fifth. This fact shows conclusively that, without an exception, each child arranged them under the influence of the illusion that their weight, in a general way, was inversely as their size. This point will be touched again in the study of the results, with special reference to the age of the children.

Comparison of the Weight of the First and Last of the Series. In order to test the amount of the illusion produced, after the weights had all been arranged as the subject thought in the order of their weight, he was asked to judge in relative terms between the one he had placed as the heaviest and the one he had judged to be the lightest. By referring to the table, it can be seen that the illusion is quite large. The judgments of the comparative weights of the two vary from one and one-third to ten. That is to say, the weights in column I were judged to be as many times heavier than the ones in column VIII as the figures indicate in the column marked "comparative weight." The average estimate of 172 children is that the weights placed in column I, which was the smallest weight in all but twenty-seven cases, was three times heavier than those placed in column VIII, which was the largest in all but twenty cases. Before these tests were made on the children, some preliminary tests had been made on adults, and it was seen that these comparative judgments were likely to be larger when all of the weights were presented for arrangement, than if only two, the shortest and the longest, were given. That is, the same weights were judged to be nearer equal when they were presented without the intervening ones. To make a thorough test of this interesting difference, 178 children of the same ages, and as near as possible of the same intelligence as those to whom all were presented, were given only the two extremes, asked to arrange them in order of their weight, and, after this was done, asked to state, in relative terms, their notion of their weights. These last children not only did not see the intervening weights, but did not know that there were any. The tables for these are not given simply because in all but *two* cases the shortest is put the heavier, and the average of the

comparisons is 2.4. That is to say, the smallest one in direct comparison with the largest one, uninfluenced by the suggestion of the intervening ones, was judged to be only 2.4 times the heavier. It is quite clear, then, that we have in the case of the judgments, when all the weights were presented, a summation effect of the illusion which, therefore, did not depend directly on the difference in the size of the two weights compared, but on the suggestion caused by the previous judgments in arranging the whole series. That is to say, the subject, after having decided that weight one was heavier than two, and two heavier than three, and three heavier than four, and so on through to eight, would come to compare the weights one and eight with the notion built up that there was of necessity much difference between them. This illusion, thus built up unconsciously, is measured approximately by the difference between these two averages, 3 and 2.4, which is .6. This means that an illusion of seventy-nine grams was built up through this series, if the largest one be taken as the standard. For, since the weights all weighed 132 grams each, the difference between these averages would give an addition of seventy-nine grams to the smallest. If the smallest one can be taken as the standard, then the largest one would be estimated seventy-nine grams too light. It seems more natural to interpret this illusion in the latter way, because the subjects all arranged the weights, beginning with the heaviest, and thus necessarily making it the standard, frequently coming back to it, indeed, as a standard as they progressed in the arrangement of the series. It is a legitimate question for the reader to ask for an assurance of confidence in this difference between the averages, or better, to ask for the probable errors of these mean results. These probable errors have been calculated according to the rules given by Jevons.[1] The error thus computed for the comparisons, when all the weights were presented, is .06; that for the comparisons when only two weights were presented is .05. The meaning of this is that if any number of judgments be made under the same conditions, one-half of them will fall between 3.06 and 2.94 in the case of the judgments when all of the weights were presented, and between 2.45 and 2.35 in the case of the judgments when only the extremes were presented. We have thus arrived at an approximate measure of the degree of credibility of the average, which indicates the region for the truth to most likely hit upon. The truth in this case being that if any number of judgments be made under the same conditions,

[1] See The Principles of Science, W. S. Jevons. p. 387.

one-half of them would fall within the limits 2.94 and 3.06 when all the weights were presented, and within the limits 2.35 and 2.45 when only the extremes were presented. Since, therefore, there is a great gap between the approaching extremes of these two sets of judgments, we are warranted in asserting with a high degree of certainty that the significant difference between the two averages is not an accidental one, but represents a difference in the judgments based on some permanent difference in the psychological basis for the judgments. This difference, as we have said, is due to the suggestion which came from the intervening judgments when all the weights were presented.

It was thought at first that there might be some relation between the size of this illusion and the least perceptible difference between weights of this heft; for if the illusion which is measured by seventy-nine grams be divided by seven, the number of comparisons made in arranging the weights, the quotient would not be very far removed from the least discernible difference between weights of this heft, were the hefts of those compared of equal ratios. But the distribution of the errors shows that there is at least no discernible connection.

The question now arises, how can this illusion be distributed throughout the series? This can be answered partially by noting the position in the series where the disarrangements have been made. It is evident that wherever the apparent difference in weight between the weights was greatest, there would naturally occur less disarrangement from their order as to size, and *vice versa*. Then, it must be borne in mind, that, as the weights increase in size, the relative difference between adjacent weights, when placed in order of their size, decreases. Thus, the relative difference between the sizes of one and two is one-third, while between seven and eight it is only one-ninth. From this relation alone we would have a right to expect fewer disarrangements near the beginning of the series than we would near the end. But since there are more disarrangements near the beginning, as can be seen from a study of the tables, the relatively greater difference in size of the first ones was more than counter-balanced by the suggestion which came when it was seen that the arrangement was probably in the order of their size. It may be well to call attention here to what will be discussed more at length further on, and that is the fact that records on forty-eight adults show the reverse to be true. That is, as we might expect from the difference in the relative sizes as the series progressed, more disarrangements were made as this relative difference decreased.

Intelligence and Apperception. In the description of the method, it was stated that before any experiments were made on the children, that their teachers divided them, on paper, into three classes, with reference to their general intelligence: class I including the brightest ones; class II, those considered good, and class III, the dullest. This classification, of course, was wholly unknown to the children, as has been said, and so in no way would affect their tests. The purpose of this was two-fold: (1) To compare the strength of the associations of the three classes, and note the influence of these on the judgments, and (2) to determine if there be any difference in the suggestibility of the three classes.

1. The strength of the associations that have been made by the subjects between size and weight is found by a study of the arrangement of the weights. In the first place, no subject hesitated in deciding that there were differences in weight, and that of quite a marked degree. Indeed, the illusion is so strong that it is impossible to rid one's mind of it, even after one sees their weight is absolutely equal, providing that he perceives either through sight or touch that there is a difference in their size.

This last fact is a very striking illustration of the strength of a long continued and practically an unbroken series of associations. Throughout our whole experience, difference in weight is associated with difference in size, and especially so when the weights to be judged are apparently of the same material, as in this case. When the weights were placed before the subject, perhaps the first observation made was that they differed in size and that some were much larger than others. The next idea which arose, especially since the test of weight sensibility was involved, was to the effect that the largest was the heaviest. These ideas perhaps all arose in the mind of the subject before he had touched a weight, so that when he began to lift them, he would unconsciously put forth effort in lifting them in proportion to their size, as he had always been accustomed to do. But as the weights were of equal heft, they of course would respond the more readily to the lift, as they were the larger, and thus arose a basis for the illusion. One would naturally suppose that after the weights had been handled awhile, that the illusion would fade away, but, on the contrary, those subjects who took much time in the arrangement of them very rarely failed to arrange them in the order of their size, judging them to be lighter as they grew larger. Indeed, after having made them and worked with them for weeks, fully conscious of the illusion all the time, I could not rid myself of the idea that there was a striking difference in their weight, when I

depended alone on what seemed to be my sensations. Some of the adult subjects, upon whom I myself experimented, after being told, refused to believe that they were of the same weight, until they saw them weighed. And even then could scarcely prevent themselves from discrediting the balances.

I have nowhere found a better illustration of the power of apperception, or how our sensations are modified and transformed by the ideas already in the mind, than is brought out in these tests. The stimulus in the case of each weight was exactly the same, but the mental result for each weight was different. That is to say, were it not for the imposition of the associations previously formed, the weights would have all appeared equal in their heft, because the same amount of muscular effort would have sufficed in each case to lift the weights.

In Steinthal's[1] story of the party in the railway carriage, all of whom were strangers to each other, it is related that one member succeeded in telling the occupation of each of the others by their spontaneous answers to the same question. Here, as in the weights, the same stimulus was applied to each person, but unlike the results in the case of the weights, the responses were very different. This brings out very clearly the fact that from the same stimulus different results arise for different people, only in so far as the associations with the same special stimulus have differed. In the case of the weights, it has been, as has been said, a universal, almost an inborn association, that of two or more weights of the same material, the heft increases with the increase in size, and so not only was the stimulus the same for each person, but the sensations received were moulded and shaped under the influence of like associations, in such a way that we get the same general results. So, while the doctrine of apperception calls especial attention to the transforming influences, brought about by the elements which the mind itself furnishes, the association processes give the key to the influences thus brought to play upon the sensations.

2. If the foregoing analysis be true, then we would expect that the more intelligent the child, the more likely he would be to arrange the weights inversely as their order in size. For it would follow that those who have been the most discriminative, not only for differences in weight, but also for size and likeness of the material, of which the bodies to be compared were composed, have the strongest and, therefore, the most dominating connection between the two. Coming to a direct study of the tables for each of the three

[1] See James' quotation in his *Psychology*, Vol. II. p. 108.

classes, the truth of these statements is fully borne out. The following table will assist in making this clear:

		Number.	Degrees of Disarrangement from the Order of Size					No. of Disarrangements.	Av. No. of Disarrangements.
			1st.	2nd.	3rd.	4th.	5th.		
I.	Girls.	30	31	9	1			41	1.3
	Boys.	22	11	4	1	1		17	.7
II.	Girls.	30	43	8	1			52	1.7
	Boys.	41	56	10	4	2		72	1.7
III.	Girls.	23	31	14	3	1	1	50	2.1
	Boys.	28	45	6	3	1		55	1.9
Totals.		174	217	51	13	5	1	287	

Explanation of the Table. Here are gathered together all of the displacements from the order of size and exhibited for the three classes of intelligence. Each class is divided with reference to sex. In class I there were thirty girls and twenty-two boys; in class II, thirty girls and forty-one boys; in class III, twenty-three girls and twenty-eight boys. In the whole set of tests there were only five grades of displacement. It will be seen that nearly all of the displacements are of the first degree. That is, they were only one place removed from the place they would occupy in the series were they all arranged in order of their size. As the table indicates, there were 217 displacements of the first degree, fifty-one of the second, thirteen of the third, five of the fourth and one of the fifth. In the next column will be found the number of displacements, and in the next the average number of displacements for each child for each class. It will not only be noticed that of all the displacements for each class, there are relatively more displacements of the first degree in class I than in class II, and more in class II than in class III, but especially it will be seen that the average number of displacements per child increases as the grade of intelligence decreases, so that it happens that the absolute number of displacements for fifty-two of the brightest children is fifty-eight, while fifty-one of the dullest make 105 displacements, making the average in the latter case double that of the former.

In accordance with this result, one would have a right to expect a still smaller number of displacements when the tests were made on adults. And this is exactly what happens. The number of displacements for forty-eight adults is only forty-four, making the average but .9 of one displacement. (See the table of records on adults page 358.)

Difference with Regard to Sex. Here, again, we might expect to find that differences in the strength of the associations would assert themselves. Since boys have to do in

their experience with size and weight relations much more than do girls, we would expect their associations to be stronger between these two contiguous ideas. This is what we find to be true, though in no very striking degree. However, by observing the preceding table, it will be seen that the average number of displacements is less for the boys than for the girls in classes I and III, and equal to them in class II. By finding the average number of displacements for all of the boys and all of the girls when the whole series of weights was given, it is found that this average for the boys is 1.5, while it is 1.7 for the girls. Though this special difference is not at all marked, it is worth noticing. The chief difference between the judgments of the boys and girls, however, is shown in the difference between the comparisons of the first and last of the series when all the weights were presented and when only two were presented. The average for the girls when all were presented, is slightly larger than the same for the boys, as is also their probable error, showing that while the girls have a distinct notion that there is a difference in weight, they have not a distinct notion of what this supposable difference is. But when it comes to a comparison of the averages of their estimates when only two were presented, there is a much wider difference. When only the two weights were presented, the boys judged the small one to be twice as heavy as the large one, while the girls made an average estimate that it was 2.4 times as heavy. This brings out plainly—since the probable errors of these means are quite small—that the boys were more influenced by the suggestion than the girls when all the weights were presented, and so in line with the fact brought out previously in the paper, that those with the stronger associations of weight and size relations were the most suggestible, as shown by the size of their estimates of their comparative weights.

Difference with Regard to Age. The 173 children upon whom tests were made for the whole series of weights, were about equally distributed for the various ages from seven to fourteen inclusive. It was expected that there would appear quite a difference between the disarrangements of the weights for the two extremes of age, as well as in the comparative judgments of the two weights. But this is not what happened in either case; the difference in each case is too slight to point to any real difference between the ideas of the two for weight and size relationship. However, since only about twenty of each age were tested, no definite statement can be made with reference to what would appear if more were tested. This lack of difference between the records of

children seven and fourteen years of age indicates that the relationship of the ideas in question is about as definite, other things being equal, at seven as at fourteen. But this result, with respect to age, will only be of value as an indication of what can now be better determined. It seems unnecessary to give the special table here, upon which these statements are based, for they can be verified from the general tables previously given.

Just here it might be mentioned that in the whole number of children tested, no less than thirteen nationalities are represented. The fathers of forty-eight per cent. were born in the United States, twelve per cent. in Germany, eight per cent. in Sweden, eight per cent. in Denmark, five per cent. in England, four per cent. in Canada, three per cent. in Bohemia, two per cent. in Norway, and the remaining ten per cent. distributed about equally in Austria, Ireland, France, Russia, India and The Netherlands.

Tests on Adults. The tests recorded in the following table were taken before the work was done with the school children, and from this work the method was partially developed and completed. It may be of interest and value to say that the records here recorded were made on subjects of various occupations, including a number of specialists in psychology, pedagogy and mathematics, as well as druggists, mechanics, grocers, and those generally whose occupation would lead them to be discriminative with reference to weight and size. By glancing at the accompanying table it can readily be seen that there are fewer disarrangements for adults than for children. In the whole number of adults, which is forty-eight, there are but forty-four displacements; thus making an average of .9 of one displacement for each subject. But especially ought it to be noticed that all of these displacements are made by thirteen subjects, and that thirty-five of them, or more than seventy-two per cent., made no displacement at all from the order of their size. It should be borne in mind that throughout this whole research with the children as well as with the adults, *one* judgment, not in accordance with the order of the size, means in nearly all cases *two* displacements. Suppose weight four was judged to be heavier than three and so arranged; there would of necessity be two displacements, according to the method used. If it were possible to make sure of the number of judgments used in all the displacements, it would be better to compare on the basis of these. But this is not always possible and so the other method has been used. If there be any preference for either class in the method used, it has been to make the difference

less marked between them, by an exaggeration of the judgments of the most intelligent, not in accordance with the order of size. So, therefore, if such a method could be used, it would only accentuate more decidedly the correctness of the results obtained by our study of the records.

No.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	Comparative weight of first and last of series.
1	1	2	3	5	6	4	8	7	
2	1	2	3	4	5	6	7	8	
3	1	2	3	4	5	7	8	6	
4	1	2	3	4	8	7	6	5	
5	1	2	3	4	5	6	7	8	
6	1	2	3	4	5	6	7	8	
7	1	2	5	4	3	8	7	6	
8	1	2	3	4	5	6	7	8	
9	1	2	3	4	5	6	7	8	
10	2	1	3	4	6	5	8	7	
11	1	2	3	4	5	6	7	8	
12	1	2	3	4	5	6	7	8	
13	1	2	3	4	5	6	7	8	
14	1	3	2	4	5	6	8	7	
15	1	2	4	3	5	6	7	8	
16	1	2	3	4	5	6	7	8	
17	1	2	3	4	5	6	7	8	
18	1	2	3	4	5	6	7	8	2½
19	1	2	3	4	5	6	7	8	2
20	1	2	4	3	5	7	6	8	2
21	1	2	3	4	5	6	7	8	1¼
22	1	2	3	4	5	6	7	8	3
23	1	2	3	4	5	6	7	8	4
24	1	2	3	4	5	6	7	8	4
25	1	2	3	4	5	6	8	7	
26	1	2	3	4	5	6	7	8	3
27	1	2	3	4	5	6	7	8	3
28	1	2	3	4	5	6	7	8	3
29	1	2	3	4	5	6	8	7	1¼
30	1	2	3	4	5	6	7	8	2
31	1	2	3	4	5	6	7	8	1¼
32	1	2	3	4	5	6	7	8	4
33	1	2	3	4	5	6	7	8	3
34	1	2	3	4	5	6	7	8	3
35	1	2	3	4	5	6	7	8	3
36	1	2	3	4	5	6	7	8	3
37	1	3	2	4	5	6	7	8	3
38	1	2	3	4	5	6	7	8	1¼
39	1	2	4	6	3	5	7	8	3
40	1	2	3	4	5	6	7	8	8
41	1	2	3	4	5	6	7	8	4
42	1	2	3	6	5	4	7	8	3
43	1	2	3	4	5	6	7	8	8
44	1	4	2	3	6	5	7	8	2
45	1	2	3	4	5	7	6	8	2
46	1	2	3	4	5	6	7	8	3
47	1	2	3	4	5	6	7	8	3
48	1	2	3	4	5	6	7	8	2

Results on Adults. It will not be necessary to explain at length what these tests indicate, for they are in line with the results obtained on the school children previously described. The method was the same in the two cases, except for the first seventeen adults who were not asked at first to make a statement in comparative terms of the weight of the first and last of their series. At this point in the work, the idea occurred, for it had become evident that the illusion was quite marked and that by this means I could get a measure of it. With the rest of the subjects, all of whom were males but two, the comparison is given. Let us first look at the arrangement of the weights. A glance is sufficient to show that there are much fewer displacements in this table than in those with the children. True it is that the twenty-two boys in class I have made a smaller average of displacements, but the probability is that if a greater number of these had been tested, their average would have been larger. Another difference between the displacements for the children and the adults has been mentioned previously; this is the difference of position in the series where the displacements occur. The following table will make this clear. Speaking generally, it is seen that the children make more displacements at the beginning of the series than toward the last, while just the opposite is true for the adults. This latter, as has been pointed out, is

TABLE SHOWING THE DISTRIBUTION OF DISPLACEMENTS.

		No.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.
I	Girls,	30	3	7	7	5	7	6	4	1
	Boys,	22	2	4	5	1	2	1	1	1
II	Girls,	30	5	7	8	6	9	7	6	4
	Boys,	41	3	10	10	12	10	8	11	8
III	Girls,	23	5	9	9	5	6	6	7	2
	Boys,	28	9	11	9	9	7	4	4	2
	Adults,	48	1	4	8	7	7	10	8	8

what one would expect from the gradually diminishing ratio of the sizes of adjacent weights. But this difference is not very striking and more records are necessary before any conclusions can be drawn from them. By far the most interesting difference that came out of the records of the children and those for adults is the greater difference which adults make

in the comparisons of the two weights when all were presented and when only the first and last were presented. The averages of the estimates when all the weights were presented is two and six-tenths, while that when only two were given is one and five-tenths. The probable errors of these means are very small, and thus render the result in a high degree credible.[1]

In brief, the results on adults show all the effects of more firmly fixed associations and hence a greater degree of suggestibility in the direction of these associations, as well as the greater influence of these in dominating and transforming the sensations directly received. That is to say, we are unable in this case to ascribe any definite value to the actual muscular sensations, simply because the preconceived notions in the mind, determined and fixed by all of our previous experience, render us unable to determine the truth in such conditions. This is a good illustration of what might be called unconscious dogmatism.

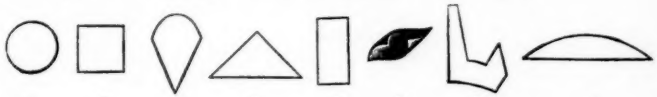
2. *On the Influence of Shape on the Judgment of a Series of Weights of Equal Weight and Size.* A series of tests was made on seventeen adults with weights of the same *weight* and *size*, but of different shape, to determine if this suggestion of difference in weight would prevail according to apparent difference in size. Eight weights were made of sheet lead, all of the same area and weight, but varied in shape from a circle to an irregular cornered figure. The table below shows that the weights which appear smaller, that is, are of a compact form, were judged to be heavier than those not so compact. While the preference, of course, is not nearly so uniform as it was for the brass weights, the averages of the numbers representing the order in which they were placed, show a decided preference. It will be seen, by reference to the table, that those weights which are judged to be the lightest are those whose form makes them appear the largest of the series. It should be noted here that when the weights were taken into the hands rather than grasped by the fingers, there was furnished a basis for a difference in the sensations of weight due to the fact that those more compact in form, for example the circle, exerted the same pressure on more closely related

[1] Since my work was completed, my attention has been called to some experiments made by Charpentier, who used two brass balls of different size, but of the same weight. He found the same illusion, the larger seeming the lighter. His balls weighed 266 grams each, but the larger was judged to be sixty-six grams lighter than the smaller. Elements de la Sensation de Poids. *Archives de Physiologie.* Série 5, Tome 3, p. 126.

A,
B,
C,
D,
E,
F,
G,
H,
I,
J,
K,
L,
M,
N,
O,
P,
Q,
Total
Aver

and fewer pressure spots, than those whose form was more irregular. The subjects were carefully observed on this point and very few of them permitted the introduction of this disturbing physiological element. Even when the weights were taken into the hands, the decision was almost invariably rendered after a balancing, of the two compared, by grasping them between the fingers. The avoidance of this physiological cause for illusion went so far with one subject (L) that he tied strings to the weights and then lifted them by the strings. Nevertheless, his record corresponds very closely to the general result.

No experiments were purposely made, either with the brass weights or the lead weights when the subjects were blindfolded, because it was thought that such a method would of necessity introduce much more strongly this probably disturbing physiological element, which was otherwise almost wholly eliminated, even when the subjects were not directed how to lift the weights, but left to use their own method, as was the case with the adults. It was, of course, wholly useless and unnecessary with either series of weights to take a series of experiments, when the elements of size and form were both eliminated, and the weight for each remained the same.



A,	1	5	3	2	7	8	6	4
B,	4	1	2	5	6	3	8	7
C,	1	3	2	4	5	8	7	6
D,	2	4	1	3	5	6	8	7
E,	3	4	2	1	6	5	8	7
F,	7	5	3	1	2	4	6	8
G,	3	6	1	4	2	7	5	8
H,	2	1	4	5	3	7	8	6
I,	2	1	5	3	4	6	8	7
J,	4	2	5	3	1	2	8	6
K,	1	2	3	4	6	5	7	8
L,	1	4	3	2	6	7	8	5
M,	1	2	3	5	4	6	7	8
N,	5	6	1	4	3	2	7	8
O,	3	2	1	6	4	8	5	7
P,	1	2	3	5	4	7	8	6
Q,	2	1	5	3	7	8	6	4
Totals,	43	47	47	60	75	99	100	112
Averages,	2.52	2.76	2.76	3.52	4.41	5.82	5.88	6.58

Explanation of Table. The figures drawn at the head of the columns roughly show the shape of the weights used. The comparative sizes must not be inferred from the drawings, as they are not exactly made. The figures in the

columns represent the order of their weight as the subjects thought. For example, the first row of figures is the order which A judged them as to weight; the second is B's order, etc. It will be noticed that the weights are arranged in the table in the order they were judged on the average. But it must not be inferred from this arrangment that they were presented to the subjects in this or any other order. They were placed before the subject in a hap-hazard way and the subject left to separate and arrange them according to his own method.

Conclusions.

1. The more intelligent the children, other things being equal, the stronger are the associations between the ideas of size and weight of a given material.
2. The stronger this associative element becomes, the more likely it is to dominate and pervert the true sensations, when the conditions are such that these associations do not hold.
3. The elements the mind furnishes, keeping these conditions, have far more influence in determining the judgment than the sensations directly received.
4. Illusions are easily built up when suggested along the lines of firmly fixed associations.
5. Consequently the brightest children are more suggestible under these conditions than the dullest ones.
6. The method used in this research furnishes a means of measuring suggested illusions of this type.
7. Adults have stronger associations between the ideas in question than children, and, despite the fact that they have a higher degree of sensibility for difference in weight, their sensations are more transformed and influenced by the element which the mind itself furnishes than are those of children.
8. Facts which vary, within limits, from our established habits of apperception are simply not taken account of at all; or, if on some occasion the conditions force us to see how our minds have become insulated against the reception of different relations, we do so with a wholly new feeling of personal fallibility.
9. The pedagogical significance of the facts emphasized in this research is of the utmost importance. It has to do with one of the most fundamental laws which regulate our mental life. The foregoing tests as a whole show how strong and dominating an association between ideas may become when they are practically unseparated and immediate in their sequence. "That which the law of gravitation is to astronomy," says John Stuart Mill, "that which the elemen-

tary properties of the tissues are to physiology, the law of the association of ideas is to psychology."

V. Minor Observations.

Reversal of Certain Cutaneous Sensations of Motion. The well-known "waterfall" illusion for vision, or as it is more scientifically called, the antirheoscopic illusion, suggested that perhaps the same illusion might occur in case of the skin, if practically the same conditions were complied with; that is, if the skin were stimulated rapidly and regularly in a continuous direction comparable to the stimulus given to the retina by the steadly falling water, or the moving stripes on the antirheoscope. To do this a frame was made sufficiently large to allow the arm to be placed in it. At each end of the frame a roller was placed and over these a belt of plaited velvet was fastened. One of the rollers was furnished with a crank, so that by turning this the skin could be stimulated with the moving folds of the velvet regularly and continuously in whatever direction desired. The folds in the velvet belt were about three-fourths of an inch in depth and one and one-fourth inches apart. Experience proved that these folds need to be made carefully and directly across the belt with no irregularities in them at all, otherwise the stimulation becomes too complex, and the resulting after sensation mixed and indistinct. Care needs to be taken, too, that as far as possible, the velvet must be kept of the same temperature as the arm, for if not, it will absorb the heat of the arm so readily as to swamp all other sensations in that of temperature. The folds of the velvet need to be pressed sufficiently hard against the skin so as to make the sensations of each fold clear cut and distinct. Having as nearly as possible satisfied these conditions, I tried the apparatus first on two women, both of whom were wholly ignorant of the purpose of the experiment, and knew nothing of the illusion for vision. I simply asked each one to give attention to the sensations caused by the moving velvet, and after it was stopped to notice carefully any sensations that might then come out different to those perceived while the belt was in motion. The result was that each one immediately said, "I feel the motion as if the belt were moving backward." Of course these trials were made so that the results of the experiment on one were unknown to the other. Afterward, I tried it upon myself and Dr. Sanford, and though each of us was somewhat skeptical as to the nature of the results, something like backward motion was experienced. It was tried for the lower arm lengthwise and crosswise, and in both directions for the

palm of the hand and for the forehead. For myself, the clearest results were obtained on the sole of the bared foot.

This sensation for reversed motion is not so clear and distinct for touch as it is in the case of vision, as would naturally be supposed, for our attention is so much less frequently directed toward sensations for touch than for sight, that we are unable to concentrate it so carefully and easily.

Although this experiment shows that the illusion in question can be produced in touch as well as in vision, no completely satisfactory explanation can be given. The following suggestions, however, may have something of truth in them and I give them merely for what they are worth.

Stimulation of any part of the body immediately causes a greater flow of blood to that part and especially do the smaller blood vessels become congested when the skin is stimulated with friction, as in the case of the foregoing experiment. Then as the folds of the velvet are swept over the skin there is a tendency for the blood to be forced along in the capillaries in the direction of the moving belt and when the actual motion stopped, to flow back again to restore the equilibrium in the vascular pressure. The rush of the blood to the face in blushing, which is very noticeable and the direction of the wave quite marked, will illustrate in a magnified degree what takes place in the skin under the conditions of the experiment. This view is in harmony with the fact that the sensation of reversal for touch as for sight is immediate, quickly dies out and does not return again as other after-images.

I am aware that the view here taken seems much more reasonable for touch than for sight, and yet the changes in circulation caused by the hastened metabolic processes in the eye brought about by such stimulation may suffice to restimulate the retinal elements of vision in a reverse order to that of the direct stimulus and thus furnish a basis for the illusion.

After-Images of Touch and Summation of Stimuli. Dr. Alfred Goldscheider[1] has called attention to the fact that if one touches the skin with a moderately sharp pointed instrument, the first sensation arising therefrom is a pricking sensation which soon dies away, but after this sensation has disappeared, a second sensation arises which retains something of the pricking sensation, but has lost the quality of touch which was prominent in the first. He found by further

[1] A. Goldscheider. *Ueber die Summation von Hautreizen.* Du Bois-Reymond's Archiv, 1891, p. 161.

experiment that if the primary sensation approached very near to a painful sensation, the secondary sensation became distinctly painful, while if the primary itself became painful, the secondary sensation was less distinct. It was found, however, that if the skin be stimulated with a single electric stimulus, the secondary sensation did not come out. It was found that, in order to arouse this secondary sensation, it required a number of electrical stimuli made on the same spot. A series of four stimulations gave a clear secondary sensation only on condition that certain intervals between the stimuli be retained. That is to say, the sensation was most marked with intervals between the stimulations of from .03 to .06 seconds. If the intervals were varied above or below these limits, the secondary sensation became less and less distinct until it gradually disappeared.

For the purpose of testing these results, I first made some small pencils of cork, gave one each to a number of subjects all in the same room, and all students of psychology, and asked them to touch their hands with these points and note carefully their sensations, allowing several seconds to elapse between the separate touches. I did not tell them what to expect, but simply to describe carefully their sensations; Goldscheider's work was known, however, to a part of them at least. The following are a few of the results obtained, and are practically the same as those on a number of other subjects: —

1(A). A more or less ticklish sensation, much like that of a mosquito bite that has nearly disappeared, at first came out. Some time after, when I supposed that all sensation had disappeared, a sharp, somewhat painful, sensation flashed out and disappeared nearly as suddenly. But after quite a period of time, in fact after writing the above description, it appeared again, but with less force each time, until it completely died away.

2(B). At first, a sense of contact and then a sense of pressure. These were both accompanied by a sensation of temperature. On touching my hand again somewhat more vigorously, I find that the demarkation between the sensations of touch and pain, is much more definite than I thought it to be. I noticed no after-sensations at all. [In later experiments, however, this subject had distinct after-sensations.]

3(C). After touching my hand with the cork, in addition to the feeling of contact, in about one-half of the cases, there came a distinct secondary sensation of a more or less painful character, and diffused over a greater area than the primary sensation. In the cases where the primary sensation was

somewhat painful, the after-sensations did not come out so clearly.

4(D). After touching the back of my hand the first time, as an after-effect there arose a slight drawing sensation; the second trial, I struck my hand a little too hard so that the first sensation was slightly painful. Later a sharp, fine sensation, somewhat extended, but chiefly located about an inch nearer the wrist, and somewhat toward the little finger side, came as an after-effect. This was very distinct, both in quality and place, from the slight pain resulting immediately from the stroke. In the third trial the pressure was slight, and the sensation of a sticking character and seemingly not so fleeting as in the other cases; it faded slowly, but not regularly, and seemed to recur several times. In the fourth trial, the touch was again very light, and the after-image was clear and somewhat irradiated. I noticed that the after-images of former touches returned and rendered this last sensation somewhat vague.

From the above results, it is clear that after-images of touch are easily recognized, and that these are of a more or less painful character, even though the primary sensation itself contain no element of pain. My results, of which only samples are given, are in accord with those obtained by Goldscheider for touch. I did not repeat his experiments with electrical stimulation to obtain the summation effects, but I found that this after-image could be much sharpened and enforced by the effects of tickle. After experimenting some time on this after-image effect, it seemed that the clearness and vigor of its effect were due largely to the plethoric condition of the part stimulated. That is to say, when the hand, for example, was cold or had been resting passively, the after-image from a given touch was much less marked than when the hand was full of blood and the attention directed toward it. It occurred to me that the tickle sensations might serve to reinforce and bring out more distinctly this after-effect. Accordingly on many naïve subjects, I first made experiments to determine the normal strength of the after-image before tickle, and then in a short time stimulated the surface thoroughly by tickling. The result, in most cases, was a distinctly marked increase in the clearness and vigor of the after-image, especially in its painful quality. No difference was noticeable, however, in the quality or quantity of the primary sensation. It would seem, then, that increase in sensitiveness for direct touch could not be assigned as the whole cause. Furthermore, I came to see that the rush of blood to the spot pressed, after the cork point had been removed, coincided in time exactly with the recurrence of the after-sen-

sation, and in my own case, and even with other subjects, I could determine precisely when the after-sensation would flash out, by simply watching the rush of blood to the spot touched. Now, it is clear that this rush of blood is the chief, and so far as I can distinguish, the only direct stimulating agent for the production of this after-sensation. Just how this is done is an entirely different thing. It may be that the temporary lack of blood in the capillaries at the point touched, caused by the pressure of the cork point, was sufficient to permit an amount of metabolism of the part, great enough, so that when the blood returned and the opposite process was started, the toxic agents released acted directly on the delicate adjacent nerve tendrils, and the result was the after-sensation. This view seems the more probable when it is remembered that the after-sensation contains invariably a painful element and none of touch or contact. It may be, and probably is, true that also the minute tissues of the part are slightly ruptured, and the return of the blood to these would thus restimulate the adjacent nerves more sharply than the original stimulus, and hence the feeling of pain. Whether this view be correct or not, the fact still remains that the return of the blood to the point stimulated, is simultaneous with the after-sensation. Goldscheider's theory, that this summation stimulus is probably due to the hindrance and the reinforcing of the stimulus in the cellular tissues, which are spread out along the nervous tract and in connection with it, seems to me to go needlessly into difficulties.

Dermographism. "Dermographism, or skin writing, is said to be dependent on two causes: an irritable nervous system and some toxic agent. Both of these conditions become causes which may act either on the peripheral vaso-motor nerves, or on the vaso-motor centre, and so influence directly the circulation." The foregoing is about what the physicians say of this. But it is very easy to obtain upon perfectly healthy subjects. In order to test this, after having found it true for myself, I asked a class of ten men to write, with moderately blunt pieces of cork, on their lower arms the word *blood*, and then slightly rub the arm. This was done, and the word in each case came out in flaming red letters after a pause of a few seconds. In some cases, there was left a welt where the cork passed over the skin, after the increase of the blood had died away, although the writing with the cork was done very lightly and gave no sensations of scratching.

The studies here described were made during the present academic year at Clark University, under the immediate direction of President Hall, to whom I wish here publicly to express my grateful acknowledgments for the many suggestions received during the progress of the work. Likewise to Assistant Prof. Sanford, who has been ever ready to render any assistance, my thanks are due. To all those also who have so generously lent their services as subjects for experimentation, I acknowledge my indebtedness, and especially to Mrs. Dresslar, without whose coöperation some of the work would have been impossible.

ON THE DIFFERENCE SENSIBILITY FOR THE VALUATION OF SPACE DISTANCES WITH THE HELP OF ARM MOVEMENTS.[1]

A. E. Segsworth, B. A. Hon. Fellow in Psychology, Clark University.

The experiments which form the basis of this paper, were commenced in the institute for experimental psychology here under Professor Wundt, in the summer semester of 1892, and continued during the following two semesters. The object was to determine the fineness of the space-sense as shown by the difference sensibility for movements of the arm in vertical directions upwards and downwards, at different heights, standards and rates of movement. The influence of the leaving of the end points determined or undetermined is also considered. The experiments consider otherwise different asymmetrical influences of the body on the valuations and certain phenomena which result from the loading of the moved members with weights. Either the two arms together or only one were moved as far as possible without bending of the elbow and hand-joints. There are two chief parts to the experiments. The first comprises such as were made by drawing simple curves upon a vertically standing table or on the wall with a soft lead pencil. These are described as the pencil experiments. They are one-handed or two-handed, according as the movements belonging to a single experiment were made with only one arm or with the two arms. In both cases the direction of the arms was forwards, parallel to the median and at right angles to the side plane of the body. The second part comprises such one-handed experiments as were made by what is here described as the angle method. For this purpose a vertically standing upwards and downwards movable table was used. On one side of this, the side adjacent to the arm, a half circle was described, whose radius was 68 cm. and at every one-fourth degree of it a hole was bored through. By this means movements of different extents in angles could be marked off by the placing of plugs in the holes. The observer sat on a chair, and the centre of the circle could be made to correspond to the centre of the arm's motion by shifting of the table up and down.

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[1] Translation in part of the original as written in German, spring and summer of 1893. See Wundt, *Grundzü.*, 4th Ed. Vol. I. p. 429. *Revue Philosoph.* Dec. 1893, p. 663. To all of those who acted as observers, I am indebted for their patience, namely, Geh. Prof. Wundt, Dr. Külpe, Dr. Kirschmann, Dr. Cohn, Mr. Kiesow, Mr. Rostosky, Mr. Child, Mr. Rogers and Mr. Hicks, as well as to the first mentioned for the conveniences of his laboratory.

I.

Experiments by Means of the Pencil Method.

The method of relative valuation and the method of relative difference sensibility.

When one investigates the difference sensibility for motion sensations, he can either leave the extent of the normal distances in any series or group of experiments determined, or he can allow the observer to fix the extent himself. In the experiments of Fullerton and Cattel[1] both methods are used. In the latter case the psychophysical methods of the average error, the mean valuation and the right and wrong cases can be used. Where, however, the normal extents, as also the compared extents, are fixed, the method of just perceptible change, the method of mean valuation and the method of right and wrong cases can be used. The fixing of the normal and compared extents brings about groups of other sensations, which complicate the judging of the pure motion sensations. These are contact, pressure and resistance sensations. Hence it is desirable to have the movements made without contact, pressure or resistance through the fixing of the extents. If, also, only the normal extents are fixed, the pressure contact and resistance at the end can make these of no use as standards for the compared extents. If one investigates the pure motion sensations, then one must, if possible, make the movements without fixing the end points. This can only take place if one makes use of shadows, photography, mirrors, or some other such optical apparatus. My first group of experiments made by the pencil method give impure motion sensations in so far as contact sensations are felt with these. Their calculation, however, is similar to such as can be made without contact. In all such movements, where the making of the standard curves equal to one another in any single series is dependent on the observer, there exists a variation of the individual from the average normal length. The influence of this variation upon the average differences between normal and compared extents where one uses the method of average error, is considered in another place.[2] Between the smallest and largest normal extents in any single series, one can further reckon out whether the individual differences upon increase of their respective normal extents increase or decrease. If we call such of the individual differences between the normal and compared extents, where the latter are greater than the former, plus; where they are smaller, minus; and where they are equal to, equal,—then the individual differences in any series can either be all plus and equal, all minus and equal, or also there can be all plus, minus and equal differences. The difference sensibility where the normal extents increased, would increase or decrease respectively with increase and decrease in the individual differences in proportion to the increase in their normal extents. If they are all minus, or minus and equal, then the difference sensibility upon increase in the normal extents would respectively increase or decrease in proportion to the increase in their normal extents inversely with the increase or decrease of their respective differences. Where these are all either plus, minus and equal, one must consider the cases where they are plus and equal and where they are minus and equal, each by itself, and from this twofold result reckon out the mean difference sensibility. The method by which the relative

[1] "The Perception of Small Differences." Fullerton and Cattel. Philadelphia, 1892.
[2] Pages 375, 379, par. 12, 381, 391d.

difference sensibility with the variations in the normal curves, where all the individual differences are plus, minus or both plus and minus, is reckoned, I call the method of relative difference sensibility. The reckoning of the difference sensibility by this method is very similar to the reckoning used in the method of relative valuation. If we let the individual differences in a series be all plus, all minus, or both plus and minus, then the valuation (judging) will increase where the compared extents increase with increase in their normal extents. This will be shown by the absolute increase of the individual differences with increase in their respective normal extents. Inversely, the valuation will decrease where the compared extents decrease with the increase in their normal distances. This will be shown by the absolute decrease in the individual differences with increase in their respective normal distances. The method by which this valuation within the variations of the normal extents in a series is reckoned out, I call the method of relative valuation. The estimation of this valuation is similar to the estimation of the difference sensibility by the method of relative sensibility, where the individual differences are either all plus or all minus. As it is the simplest, I give at first the reckoning out of the method of relative valuation.

Description of the Reckoning Out of the Method of Relative Valuation.

We let m_1 m_2, m_3, etc. . . . . m_x be the simple digits 1, 2, 3, etc. . . . . x, of which m_1 represents the greatest normal extent, m_2 the one in the same series which is just smaller than m_1, and so on up to $m_x = x$, which represents the smallest normal extent. We let further n_x, n_{x-1}, n_{x-2}, etc. . . . . n_3, n_2, n_1, be the same digits x, x — 1, x — 2 . . . . 3, 2, 1, of which, however, $n_x = x$ represents the smallest difference between any individual normal extent and its compared extent in the same series, $n_{x-1} = x-1$, that which is somewhat greater than n_x up to $n_1 = 1$, which represents the greatest difference between any normal extent and its compared extent in the series. We add, then, each m value to its respective n value, so that the m value, which represents any individual normal extent, is added to its respective n value, which represents the simple difference between this normal extent and its compared extent. r, o, s, etc. . . . . g are the sums of such individual values as $m_1 + n_x$, etc. . . . . $m_x + n_1$, according as the valuation takes place. In every series there exists x such values. d, e, etc. . . . f are the variations of these values from $(m_n + 1) = (n_1 + 1)$. q represents the average of these variations. If m_1, m_2, etc. . . . . m_x are added respectively to n_x, n_{x-1}, etc. . . . . n_1, then will d, e, etc. . . . . f each be like null and the average variation q will also be like null. If they are added together inversely, namely m_1 to n_1, m_2 to n_2, etc. . . . . m_x to n_x, then the average variation will be Q. This Q gives us the greatest possible average variations $\frac{q}{Q} \times {}^{100}$ gives us a value which shows whether the differences between the normal and compared extents increase or decrease with the increase in their normal extents, *i. e.*, whether the valuation increases or decreases with increase in the normal extents. If $\frac{q}{Q} \times {}^{100}$ is equal to 100, *i. e.*, $q = Q$, then in comparison with the valuation of the smallest compared extent, a gradual increase of the compared extent with reference to its normal extent would be shown with increase of

this last in the same series. If $\frac{q}{Q} \times {}^{100}$ is equal to 0, *i. e.*, $q = 0$, then a similar gradual decrease would be shown. If $\frac{q}{Q} \times {}^{100}$ is equal to 50, then there would be shown on the average for the series neither such a decrease nor such an increase. If the value of $\frac{q}{Q} \times {}^{100}$ lay between 0 and 50, then there would be more or less of a decrease; if between 50 and 100, more or less of an increase.

Description of the Reckoning Out of the Method of Relative Difference Sensibility.

If all the individual differences between the normal and compared extents are plus, then the difference sensibility, where the value obtained by the method of relative valuation is 0, increases with increase in the normal extents; where 100, decreases; where 50, it neither increases nor decreases on the average; where it is between 0 and 50, it more or less increases, and where between 50 and 100, it more or less decreases. If the individual differences are both plus and minus, or equal and minus, then it is necessary to reckon out the value obtained by the method of relative valuation for the different normal extents which show plus or plus and equal differences between the normal and compared extents, and likewise for the different normal extents which show minus or minus and equal differences. The reckoning out of this latter will, however, be different from the reckoning in the case of the method of relative valuation only in so far as one allows $n_x = x$ to represent the difference, which is nearest the value null, $n_x - 1 = x - 1$, that which is somewhat smaller, and $n_1 = 1$, the minus difference, which is most unequal to the simple difference null. The values of q, Q, and $\frac{q}{Q} \times {}^{100}$ are now reckoned out, and also the mean of the two values, namely, that obtained from the normal extents which give plus differences and that obtained from those which give minus. If this value is null, then the difference sensibility gradually increases with increase in the normal extents; if it is 100 it gradually decreases; if it is 50, then it neither increases nor decreases on the average; if it is between 0 and 50 it increases more or less, and if between 50 and 100 it decreases more or less.

It is here to be remarked that when we speak of the normal extents in any single series, from the standpoint of difference sensibility, we have to do with an absolute increase or decrease in the series and not with a relative. Only when we compare this increase and decrease in one series with the same in another can we speak of a relative difference sensibility. If one wished to estimate the relative difference sensibility for each series alone, then instead of using the individual differences to reckon out the n values, he must use these divided by their respective individual differences. This, however, has little value. In the same way, we can only speak about an absolute increase or decrease in the valuation, unless we use, in reckoning out the n values in the method of relative valuation, instead of the individual differences, these divided by their respective normal extents. This has also, however, insufficient value. We can, however, speak about a relative valuation or a relative difference sensibility in connection with the values obtained by these methods, in so far as in the case of the first method one takes the valuation where the normal extent is smallest, as normal valuation for the series, and in the case of the other, the difference sensibility

as shown, where the individual difference is null, or nearest above or below null, as the normal difference sensibility in each series. The values obtained by these methods are dependent on this normal valuation and on this normal difference sensibility, and to some extent show a relative valuation and a relative difference sensibility. From these considerations we obtain four methods which show connections between the individual series and the increase in the normal extents, as well as the increase or decrease connected with this in the increase and decrease in the valuation and in the difference sensibility, namely, the two which we have above completely described, the method of relative valuation and the method of relative difference sensibility, and two others which correspond to these, and which give values for the increase and decrease in the relative valuation and the relative difference sensibility in the individual series, instead of the absolute valuation and the absolute difference sensibility, in the individual normal extents. These two latter methods we do not consider further.

The reckoning out of the method of relative valuation and of the method of relative difference sensibility is accompanied by certain difficulties which can be overcome only through carefully choosing the number of experiments and the digits. In the first place, if there is more than a single normal extent, of the same value, one must take the mean of these and also of their respective differences and reckon out the results in the ordinary way with this reduced number of experiments in the series. The same can also be done where there is more than a single simple difference in the same series of the same value. In the second place the average value "q" might be the seat of an error in that the individual parts of $V(m_x + 1)$ work contrary to one another. A table (Part II) with an instance where this occurs is given below, where 2.40 is = q = Q.

Part I.			Part II.					
$M+N$ =	sum	$V(m_x+1)$	N L	D	M	N	M+N	$V(m_x+1)$
m_1+n_x	r	d	20.50	— .50	3	1	4	2
$m_2+n_{(x-1)}$	o	e	21.50	—1.50	1	2	3	3
$m_3+n_{(x-2)}$	s	etc.	21.25	—2.00	2	3	5	1
etc.		etc.	18.25	—3.75	4	4	8	2
m_x+n_1	g	f	17.50	—4.00	5	5	10	4
$5 \mid \frac{r+\text{etc.}+g}{m_x+1}$		$5 \mid \frac{d+e+\text{etc.}+f}{q}$						$5 \mid \frac{12}{2.40}$

But it is easy to see that it should be smaller. This error is not frequent and it would serve no present purpose to reckon it out, in so far as no wholly exact value is sought, but only a relative one.

The method of relative valuation and the method of relative dif-

ference sensibility will be used as an aid in the reckoning out of the difference sensibility in the experiments made by means of the pencil method, and is represented by the symbols M. R. V. and M. D. S.

A.

The One-Handed Movements.

1. A General Consideration of the First Experiments.

I have reckoned out these experiments all together, without taking into consideration the length of the curves. The results show that out of a total number of experiments of 1008, $1\frac{1}{4}$ per cent. were overvalued, $\frac{1}{4}$ per cent. were valued correctly, and $18\frac{1}{2}$ per cent. were undervalued. Each of the sixty-six series was at first treated separately. The average differences of the experiments, where over, under and equal valuations took place, along with the average variation of these, were reckoned out for all the experiments. These were then grouped into combinations of six and seven series in a group. These groups were then calculated all together. The average of the whole of the averages of the normal and compared curves, of their differences, of their variations, of the experiments of the sixty-six series, was then reckoned out. In each of the sixty-six series there were overvalued experiments, and in fifty-one of them there were experiments where undervaluations took place. The number of experiments, where correct valuations took place, was so small that we shall not take them into consideration here. The lengths of the normal curves varied between 10 and 60 cm. The average of all the normal curves, where an overvaluation took place, is 22.31 cm. The same, where an undervaluation took place, is 29.62 cm. The average of the differences of the former is 5.48 cm., of the latter, 3.53 cm. When both the normal and compared curves are added together, the average obtained in the first place was 25.96 cm., and in the second, 26.93 cm. In this way one obtains an average difference in valuation of .97 cm. The average of all of the average normal curves of each series, without consideration of the over and undervaluation, is 23.59 cm. The arithmetical average of all the average differences was 2.75 cm. overvaluation, an average difference sensibility of about $\frac{1}{10}$. It is seen, then, that both according to the number of experiments, and also to the average differences, a significant overvaluation had taken place. A review of the series shows that nineteen series had an average normal curve of between 10 and 15 cm., twenty of between 20 and 30 cm., fifteen of between 30 and 40 cm., four of between 40 and 50 cm. and two of between 50 and 60 cm. The average of the average variations of fifty-one series, where an overvaluation took place, was 2.37 cm. For thirty-seven series, where an undervaluation took place, it was 1.00 cm. As this was small in such a general consideration, I have not taken it into account.

It is desirable now to find out what sort of influences the upwards and downwards direction of the motions, the changes in the normal lengths and in the height of the starting points, as well as special differences in the observers and in the methods of experimenting, could show. In the case of the first fifteen series the height of the starting point was not taken, and we do not consider these further. Instead of a lead pencil being used, also a small brush with lamp black mixture was used. Of the remaining fifty-one series, thirty were made in the upwards direction[1] and twenty-one in the

[1] See table II. p. 407.

downwards.[1] The accompanying tables[1] show the heights of the starting points from the lowest, which is marked with minus, to the highest, which is marked with plus. The horizontal starting point, which could be only roughly estimated, is marked with zero.

In considering this first group of experiments it is important to remark that they were all made more with respect to the convenience of the observer than to any previously determined plan of experimenting. The beats of the metronome, which regulated the rate of movement as well as the length of the curves, show also to some extent the disposition of the observer apart from the exactness of his judging. Very quick time and very slow time was disagreeable. A look at the diagrams[2] shows also that the average lengths of the normal curves for the series varied more under the horizontal point in the case of the upwards movements, but in the case of the downwards movements more over the horizontal point. Not only also are the limits for the average normal lengths greater in these directions, but the number of series is also greater in the case of upwards movements under the horizontal point and in the case of downwards movements above the same. Apart from series 20 of the upwards movements and series 17 of the downwards, a certain attempt was made to make all the normal curves in each series as much as possible equal to one another. There was nothing binding in this, however, as it was thought that it could possibly disturb the power of valuing correctly between the normal and compared curves. Still a certain indirect valuation in this respect was present in each series. Each new normal curve was more or less of an attempt to reproduce the former. In so far, such a valuation was implicated. The variation of the normal curves gives a value for this. In the case of the upwards motions, the changes in the variations follow the changes in the lengths of the average normal lengths, with few exceptions. In the case of the downwards motions this is also very often the case. This shows that the difference sensibility is dependent on the length of the normal curves. In many cases also, the normal curves were made of the same length as the just previously described compared curves. In such cases the compared curves were made smaller than their normal curves. These two pairs of curves appear, then, to form a separate basis of valuation. The above described average values of all the series can be considered as a sum of such valuations, of which the average differences show the exactness of the valuation. As the average overvaluation of the experiments of all the 66 series, where overvaluation took place, is 5.48 cm., the corresponding average undervaluation 3.53 cm., and the average overvaluation in all the experiments was 2.75 cm., we see that the difference sensibility would amount to about $\frac{1}{20}$. +23.59 cm. is the average normal length of all the experiments; $\frac{2.75}{23.59 \times 2} = \frac{1}{20}$ = difference sensibility. In general, the curves show no very exact changes caused by changes in the lengths of the normal curves and in the starting points. The larger normal curves show in the case of the upwards movements, where the starting points were, —30 and —60 cm. larger average differences than do the smaller normal curves. In the case of the downwards movements, the average differences show the opposite results, so that the smaller normal curves are more overvalued than the larger. It is easy to see that on account of the changes in the average normal lengths and in their variations, the influence of the changes in the starting points in these experiments can not be

[1] See table I. p. 406.
[2] See original.

very exactly determined. Where also the variations of the average normal curves and the changes of the starting points do not vary much, the variations of the average differences are too great to give exact results. In order to find out the influence of the starting points, I have made the two-handed experiments, which are given below (B), and I have also made other additional one-handed experiments to determine the influence of the extent of the normal curves. I give in addition certain remarks on the individual series. By the method of relative valuation above described, I have reckoned out in the individual series the tendencies towards under and over-valuation. From this it appears that in the case of upwards motions, where the starting points are very low or very high, an undervaluation takes place more and more with the increase in the length of the normal curves. Under and at the horizontal point, however, more of an overvaluation takes place. These over-valuations and undervaluations are not very much dependent either on the changes in the average normal lengths nor on the changes in the variations of the individual normal curves from their average normal lengths. In the case of the downwards motions with low starting points, a distinct undervaluation of a similar kind takes place, and is also present where the starting points are taken quite high. Only when the starting points are very high is an overvaluation met with.

2. *Consideration of the Individual Series.*

a. Series 11 and 12 of the upwards motions and series 8 of the downwards were made while the observer was seated. They show no special deviations from the other series. Among the later experiments are some which show more particularly differences between the standing and sitting posture.

b. With and without the beating of the metronome. Series 16, 24, 26 and 5 of the upwards motions were carried out with no accompanying beating of the metronome. It is to be remarked that the variation of the normal curves in each case is very small. The angle method experiments to be described later will show the more important influence of the rate of movement upon the relative valuation. In general it appears that the observer without the beating of the metronome, chooses the most convenient rate of movement for the starting point and length of movement. The metronome's beating has less influence on the variation of the normal lengths with such as are accustomed to see the regular motions of machines, or to make regular motions themselves. In these cases the two groups of sensations, the normal and the compared, are more or less localized in the organs of motion. The motions are automatically made. Where this is not the case the metronome exercises a greater and more distinct influence upon the variation in the normal curves. Along with this the two groups of motion sensations can be considered more as sensations of the willing impulse, which are localized in the glottis and in the breathing apparatus. As the metronome tends to make these motions also automatic, however, these sensations are not so important (as materials in investigating) as in either case where one makes movements of unusual length and height.

c. Influence of the lengths of the radii. The irregularity of many of the average differences which are dependent neither upon the differences in the variations of the normal curves, nor upon the differences in the heights, can be very well ascribed to the bending

of the arm. It is desirable, therefore, to find out what relation the radii of the normal curves bear to the radii of the compared ones. The curves were more or less bent towards the extremities. The measuring of the radii, then, was confined to the normal arcs in the middle of the curves. A large wooden compass was used to measure these directly. Where the radii of the normal curves are smaller than those of the compared, the differences are marked with plus, where larger with minus. In the case of the upwards motions, it appears that the normal radii are smaller where the starting points are very high and very low than where the points lie between these extremes. It is meant that the variations in the lengths of the normal radii and the average differences between those of the normal and compared, show the value of this measuring. Where one variation is relatively small, so is the other, where large, likewise. From this it follows that there existed regular fluctuations in the bending of the arm. The bending of the arm alternates more or less in each series, and the average differences of the radii show, then, how the valuations of the movements are influenced through this.

If we consider now more particularly series 2, 5, 6, 7 and 9 of the upwards movements, which have starting points very nearly alike, we find that by increasing the height of the starting point as well as by lengthening the average lengths of the normal curves, the lengths of the average radii of these increase. If we consider, however, only the changes of the lengths of the average differences of the radii in connection with the changes in the average differences of the curves, then we find that they stand in a directly inverse relation to one another. The exception in series 6 can be very easily ascribed to the greatness of the variation.

In series 1, 5, 6 and 11 of the downwards motions, the lengths of the average normal radii increase with the heightening of the starting points. The first three series were made under the horizontal point and can be considered by themselves. The average differences of the normal and compared curves stand in a directly inverse relation to the average differences of their radii. Apart from the starting point the increase in the average lengths of the curves is accompanied by a decrease in the average differences of the normal and compared curves, and by an increase in the average differences of the radii themselves.

In the case of both upwards and downwards movements, then, it results that an increase in the average differences of the normal and compared curves is accompanied by a decrease in the lengths of the differences of their radii. The greater normal curves are, however, more overvalued in the former case than in the latter.

d. What relation do the curves bear to their corresponding angles and perpendiculars? I have drawn diagrams[2], which give the average values for the above described series. For series 2, 5, 6, 7 and 9 of the upwards motions, the average of the heights of the starting points was —52.10 cm. The average of the average normal curves is 26.67 cm.; of the compared 34.10 cm.; of the average radii of the first 56.72 cm., of the last 61.96 cm. In the case of the downwards movements, the average of the heights of the starting points for series 1, 5 and 6 was —42.87 cm. The average of the average normal curves is 16.96 cm.; of the average compared 17.65 cm.; of the radii of the first 54.19 cm., of the last 56.63 cm. From these values I have reckoned out the corresponding angles and the perpendiculars to the horizontal plane lying between the extremities of the curves. Dr. J. Loeb[1] has shown

[1] Archiv für die ge. Phy. d. M. u. d. Th. von Pflüger. Bd. 46, I. S. 1; Bd. 41, S. 107.
[2] See original.

that where the starting points lie under the horizontal plane, motions made at right angles to this plane show great deviations between the normal and compared distances, and this just so much the more that the starting points are distant from the horizontal plane. If the observer had described such straight lines without changing the angles of movement, the above perpendiculars would show my results to agree with those of Loeb's. My experiments show, however, that in order to make lines of equal greatness, the angles at the shoulder should be taken as the standard and not the perpendiculars. In both cases, whether one makes movements with the arm held straight out in the form of curves or with the elbow joint bent in the form of straight lines, it is to be assumed that the mass of sensations are so nearly equal to one another that they do not need separate consideration. There exists a probability for this in so far as the contact and holding of the elbow joint when the arm is fully stretched out, compensate for the other kinds of stimuli which arise through the bending of the elbow joints and the muscles. How far the sensations due to the moving muscles and to the so-called will-impulse are to be taken as the basis for the valuation, will be explained later. The chief point to be noted here is that the angle at the shoulder joint (or joints) is to be taken as the basis for the valuation of the distance moved. The above described changes in the perpendiculars bear a similar relation to their corresponding angles at the shoulder in the case of the upwards movements to what 8.12 cm. and 26.42 cm. bear to 26° 56′ and 31° 34′, and in the case of downwards movements to what 9.25 cm. and 5.72 cm. bear to 15° 53′ and 17° 48′. These two accompanying diagrams[1] give us examples as to how these perpendiculars stand to their curves or angles at the shoulder. This relation changes with the starting points. In the horizontal plane both stand in a very close relation to one another. High above and low below, the greatest deviation between the two occurs. Loeb used the two arms in his experiments. The normal arm had always the horizontal plane as starting point, while the starting point for the other arm was varied.

3. The Later One-Handed Experiments of the Winter Semester, 1892-3.

As above mentioned I have made further experiments with special respect to the changes in the average differences which arise through changes in the lengths of the normal curves and in the heights of the starting points. The latter I have considered more especially in the two-handed experiments to be described later. The experiments we consider here, consisting of ninety-nine series, were all one-handed. In twenty-nine series the starting points were changed. In all the ninety-nine series an attempt was made as much as possible to keep the variation of the normal curves for each series small. The number of experiments for each series was ten. The observers were Mr. Child, "C.," and the experimenter himself, "S." The former was seated, while the latter remained standing and acted as his own experimenter. The horizontal plane for the former was 102.5 cm. above the floor, and for the latter 130 cm. above the same. The number of series made by the first was nine in the upwards direction and eleven in the downwards; by the last, forty-five in the former and thirty-four in the latter. The beating of the metronome was, with the exception of eleven series, by observer S., where it made 180 beats in the minute, and five others where it made 120, kept at sixty beats to the minute.

[1] See the original.

(1.) *Observer C.*—The series by observer C. were made, with two exceptions, in order to find out the changes in the average differences caused by changes in the lengths of the average normal curves. In general the average differences were not nearly as large as in the earlier experiments. The variation in the normal curves follow in their relative values the changes in the lengths of the average normal curves. Where the latter decrease, so do the former. With the same decrease the average differences in the case of the downwards motions also decrease, and inversely in each individual series the increase in the individual differences is accompanied by an increase in the lengths of the normal curves.

Upwards movements. The accompanying tables[1] show the two groups of series of upwards motions by observer C. Series 4, group II., had, besides the starting point in the horizontal plane for the normal curves, a different one for the compared. It is, then, left out of consideration here. The question presents itself here, "How does the relative and absolute difference sensibility decrease or increase with the increase in the lengths of the normal curves?" The variations of the normal curves and the differences of the normal and compared curves can only be considered as absolute spatial changes, which serve as a basis for an absolute difference sensibility. With the increase in the normal curves they also increase. Hence the absolute difference sensibility decreases. The M. R. V. shows that where the individual differences in each series are considered, each increase in the normal curves, where these are greatest, is accompanied by a decrease in the individual differences; where they are smallest by an increase, however. The average differences show that with increase in the average normal curves the average differences increase in group II., and inversely in group I. decrease. Were the average differences not reckoned out, one could estimate the difference sensibility to a certain extent by the method of relative difference sensibility. The figures obtained by this method show that in group I. the difference sensibility increases with the increase in the individual normal curves, and this just in proportion as the individual normal curves decrease. In group II., however, it decreases, and just in proportion as the normal curves increase.

Downwards movements. Two groups of experiments were also made by observer C in the downwards direction. The M. R. V. shows a constant increase of the absolute values of the individual differences in the series with the decrease in the normal curves, and this the more the average normal curves increase. The average differences in the two groups are themselves different. In series 1-4, group II., they are greater than in series 1-8, group I. The difference sensibility is then inversely greater with the latter. In both groups the increase in the lengths of the average normal curves appears to cause neither an increase nor a decrease in their average differences. They do not allow of a judgment in regard to the absolute difference sensibility. In both groups the norm al variations decrease also with decrease in the average normal curves. As above mentioned there is ground here for concluding that the absolute difference sensibility decreases with the decrease in the lengths of the average normal curves.

Changes in the starting points. The two remaining series of downwards movements of observer C. show with increase of the starting points a greater decrease of the compared curves in relation to their normal. The absolute difference sensibility was great-

[1] See the original.

er above than below, because the differences in both cases were greater. This agrees with what was said in regard to the other downwards movements.

(2.) *Observer S.*—Upwards movements. The six groups of upwards movements of observer S. were made under different conditions. In all of these, apart from group V., the variations were greater than where the average normal curves were greater, and smaller where these were smaller. Hence the difference sensibility decreases with increase of the normal curves. The lengths of the averages of these vary between 5.95 cm. and 53.95 cm. The average differences in group I. show no regular increase or decrease, with the increase in the lengths of the average normal curves. The average of the last three series is greater than that of the first three. This shows a finer difference sensibility for the greater curves than for the smaller. The dividing of the average of the compared curves by the same of the normal ones shows the difference sensibility from 5.95 cm. to 23.10 cm. to gradually increase. At 30.15 cm. it is again less and at 44.70 cm. is about the same as at 23.10 cm. The M. D. S. shows that the difference sensibility, as shown within the limits of a group, increases on the average with increase in the individual normal curves.

Group II. of observer S. was different to group I. only in so far as the metronome beat 180 times in the minute instead of sixty. By this the rate of the movement was considerably increased. In general an overvaluation took place, and this is greater with the larger normal curves and smaller with the smaller. The variations of the normal curves are also greater with the greater average normal curves and smaller with the smaller. Series 3 forms an exception to this. The normal variation of the same is greater than the normal variation in series 2. From the standpoint of the average differences and of the average variations of the normal curves, there appears a greater absolute difference sensibility when the normal curves are smaller than when they are larger. The M. R. V. shows in general in the individual series a decrease in the individual differences with increase in the individual normal curves. This decrease is somewhat larger when the normal curves are greater than when they are smaller. In series 2, 4 and 5, the M. D. S. shows that the difference sensibility in the average decreases with increase in the individual normal curves, while in series 1 and 3 it decreases.

Group III. was made in such a way that in each series four curves of different normal lengths were made after one another in such a way that the second was greater than the first, the third greater than the second, and the fourth greater than the third. Each time it was attempted in the following sixteen series to repeat these four experiments with similar normal lengths. For the first twelve of these, the metronome beat sixty times in the minute, and for the last five (group V.) 120 times to the minute. In groups III. and V. the variations are relatively greater than in the other groups of these experiments. They follow, however, in size the lengths of the average normal curves. With the larger ones they are larger and with the smaller they are smaller. We obtain, then, a greater absolute difference sensibility in the case of the smaller ones than in the case of the larger ones. The average differences themselves are smaller with the larger curves than with the smaller curves. An undervaluation takes place with the former and an overvaluation with the latter. This overvaluation is greater than the undervaluation. From the standpoint of the average differences the difference sensibility is less in the case of the larger normal curves than is the

case of the smaller. The M. D. S. shows in the individual series that the individual differences where the average normal curves are large, decrease with the decrease in the individual normal curves, and where the average normal curves are small rather increase. This agrees with the results obtained from the average differences.

Group IV., where the metronome beat 120 times in the minute, shows in many ways results similar to those in group III. The average differences vary with one another somewhat more, and in series 2 the variation of the normal curves is greater than would be expected from group III. In series 3 it is smaller.

Group VI. was made so that the normal curves in series 1, 2 and 5 were made without rubbing the papers. The compared curves were described in the ordinary way, with the lead pencil. The M. R. V. shows that the increase of the individual normal curves in the case of the larger normal curves is accompanied by a decrease in their differences; in the case of the smaller by an increase. The variations of the normal curves show in series 5 a small variation, in series 1 and 2 a large one. From the standpoint of the variations the difference sensibility decreases with increase in the average normal curves. The average differences in series 1 and 2 give a mean which is smaller than the average difference in series 5. From the standpoint of the average distances, then, it is to be concluded that with increase in the average normal curves, the absolute and relative difference sensibility increases. This agrees with the results from the method of relative valuation. In general these series show no great deviation from group I. The rubbing in the case of the normal curves appears to exercise no great influence upon the valuation. Series 3 and 4 were made so that the arm was fatigued through lifting of a weight of five kg. It shows, however, nothing new.

Group V. was carried out from the standpoint of the variations of the compared curves. The normal curves were made in such a way that the eyes could see only and alone the making of the normal curves. By this it was thought to keep the pure stimulation or the normal curves more constant and to make the average differences follow more the real differences in the length of the motions or of the space valuations. The average normal curves varied in the 8 series between 6.17 cm. and 30.30 cm. Their variations give no regular increase or decrease with the same in the normal curves. The average of the larger average normal curves is greater than the average of the smaller. From this standpoint, then, the difference sensibility is greater in the case of the latter. It is, however, much smaller than in the other series. From 12.95 cm. to 15.79 cm. the average differences rise. From 20.65 cm. to 30.30 cm., and from 12.95 cm. to 6.17 cm., they decrease. In general the larger normal curves show much larger average differences than the smaller. The M. R. V. shows in general a small increase in the individual differences of each of the series with increase in the individual normal curves. In three series it is just the reverse. In the other five, however, it is so much to the contrary that an average increase takes place. The relative difference sensibility obtained from the average differences is greater in the case of the larger normal curves than in the case of the smaller. An exception to this occurs in series 6, where it is greatest. We find by the M. D. S. that the difference sensibility increases, in series 3, 4 and 7, with increase in the individual normal curves. In series 1, 2, 5, 6 and 8, however, it decreases.

Downwards movements. I consider now the downwards movements of observer S., by which changes in the difference sensibility caused

by changes in the lengths of the normal curves are sought. Only two of the twelve series had a more rapid rate of movement, namely, 120 beats of the metronome in the minute. In the case of the others it beat sixty times. The average normal curves varied between 2.14 cm. and 23.25 cm. Their variations increase gradually with increase in their lengths. From this standpoint, then, the absolute sensibility decreases gradually with the increase in the lengths of the average normal curves. The average differences, on the contrary, also gradually decrease with increase in the normal curves. On the whole they are very small. The greatest difference sensibility is at 11.50 cm. and at 4.40 cm., where the average differences are respectively —.05 cm. and +.05 cm. From these normal lengths it decreases with the longer and shorter normal curves. At 23-25 cm. the difference sensibility is greater than at 2.60 cm. (the average of series 5-9). Within the limits of the individual series the M. R. V. shows that a decrease in the lengths of the individual normal curves is accompanied by an increase in the size of the average differences, and this in proportion as the average normal curves increase. Series 11 and 12, which were made with the quicker rate of movement, show an increase in their average differences, and in their average normal variations above motions of the same size which were made with a slower rate. One series of the downwards movements, in which the normal curves were made without rubbing of the surface, gave an average normal length of 36.40 cm., with a variation of 4.35 cm., and an average difference of 12.77 cm. The contact of the hand with the table through the lead pencil appears, in the case of the downwards movements, to exercise a great influence. The M. R. V. shows an increase in the individual differences with decrease in the individual normal lengths.

Still another series was completed after fatiguing of the arm with a five kg. weight. The average normal length was 19.04 cm., their variation 2.47 cm., and the average difference 4.18 cm. The fatiguing brought out an overvaluation in comparison with series 3. In these downwards movements fatiguing causes the difference sensibility to decrease, both from the standpoint of the variation of the normal curves and from the standpoint of the average differences. The M. R. V. shows that an increase in the lengths of the average normal curves is accompanied more by a decrease in the average differences than by an increase.

The series so far described were made so that the starting points of the compared curves were the end points of the normal. Six series were now made in which the starting points for both normal and compared were the same. The lengths of the average curves varied between 8.12 cm. and 58.82. The variations and the normal curves themselves vary more in the case of the series considered above. They show no increase with the increase in the length of the average normal curves. The average differences are not very much different from the average differences of the other series. The greatest difference sensibility is at 17.90 cm. Over and under this it varies. It is least where the normal lengths are very great. The average differences vary between 0.00 cm. and —2.42 cm. In only one series is there an overvaluation. At 41.60 cm. the average difference is +1.90 cm., at 58.82 cm. it is 2.42 cm., and apart from these an undervaluation of from 0.00 cm. to 1.00 cm. is always present. The M. R. V., apart from series 1, where the average normal curve was 58.82 cm., shows an increase of the individual differences with increase in the individual normal curves.

Experiments with changes in the heights of the starting points. So far, we have considered these later results from the standpoint

of the lengths of the normal curves. As above mentioned, I have also carried out some such series to determine how far changes in the average differences were due to changes in the length of the normal curves. In the downwards direction thirteen series were made in two groups. In group I. the average normal curves varied between 13.75 cm. and 15.70 cm. Their starting points varied between 60 and 15 cm. under the horizontal plane. The average differences are for the lower of these, smaller, and for the higher, greater. The variations of the normal curves are greater in the first than in the last. The variations of the normal curves, then, show a smaller absolute difference sensibility at the lower points, while from the standpoint of the average differences a greater at these points. The M. D. S. shows more of a decrease of the difference sensibility with the increase in the lengths of the individual normal curves. The M. R. V. shows an approximate equal increase of the individual differences with increase in the lengths of the normal curves more and more as the starting points are raised.

Group II. consists of nine series, of which two are duplicates. The starting points vary between —45 cm. and +60. The variations of the normal curves all fall under 1.50 cm., and are somewhat greater above than below the horizontal point. The average normal lengths vary between 7.40 cm. and 12.95 cm. The larger of the same are the upper ones. The average differences are greater under than above the horizontal plane. In the former case there is an average overvaluation, in the latter an average undervaluation. None of the average differences amount to more than +1 cm. or to less than —1 cm. The absolute average undervaluation in the higher series is greater than the absolute average overvaluation in the lower ones. The M. R. V. shows in the former series an increase of the individual differences with the increase in the individual normal curves, while in the latter series a decrease is shown. In these latter series, we find that the increase in the average differences agrees with the increase in the individual differences, as shown in the separate series. The M. R. V. shows in the horizontal plane a decrease in the difference sensibility to be accompanied by an increase in the individual normal curves. With the heightening and lowering of the starting points this decrease becomes less, so that an increase takes place. On the average, also, there is an increase. Groups I. and II. are different from one another in so far as the first shows, on the whole, a larger overvaluation than the second.

Two groups of series of these kinds of movements were made in the downwards direction. In group I. the lengths of the normal curves varied between 8.22 cm. and 11.65 cm. Their variations are greater at the higher starting points, namely, 0.00, +15 and 30 cm. than at the lower, namely, —45, —30 and —15 cm., and also than at the highest, namely, +60 and +45 cm. The M. R. V. shows that the increase in the lengths of the individual normal curves at the lower points is accompanied by an increase in the lengths of the individual differences, but at the higher points by a decrease. The average differences varied between —1.00 cm. and +1.75. The average differences at points —45 cm., —30, —15 and at 0.00 are +1.17, —.55, —.87 and —.02 cm. At the points +15, +30 and +45, they increase. They are +1.36, +1.50 and +1.75 cm. At +60 it is —.9 cm. The variations of the lengths of the normal curves appear to have no influence on these average differences. We can assume, then, from these experiments that the difference sensibility is smaller above the horizontal plane than below it. Apart from the lowest point, —45, it is greater under. In group II. the normal

curves are longer. They vary between 16.72 cm. and 21.42 cm. The starting points were +15, +45 and +60. The variations are much the same as in group I. The average differences decrease with the increase in the lengths of the average normal curves and in the heights of the starting points. Along with this the difference sensibility decreases.

This finishes the results obtained from the one-handed experiments by means of the pencil method. The results are:— 1. The results obtained by the pencil method show that in valuing space distances, the angles at the shoulder joint should be taken as the measure of the sensibility in the making of arm movements.

2. The larger normal curves, when the movements are upwards, are more overvalued than are the smaller, where they are downwards more undervalued. This varies, however, with the observer and with the rate of movement.

3. The results obtained by the M. R. V. show that the individual compared curves increase on the whole with increase in their normal curves in both upwards and downwards movements. When the normal curves are small they rather decrease, however. This relation between the normal and compared curves is dependent also on the length of the normal curves, as also on the heights of the starting points.

4. The variations of the normal curves show that the absolute difference sensibility is greater where the normal curves are smaller than where they are larger, because the variations are greater in the latter case than in the former.

5. The upwards movements at points above the horizontal plane are overvalued, and under it they are undervalued. The reverse holds where the movements are downwards. This over and undervaluation varies more in the former than in the latter movements. Observer C., who was seated during the experiments, overvalued upwards movements above the horizontal plane and undervalued downwards movements under the same plane.

6. Very rapid rates of movement caused greater average differences in the larger and smaller normal curves than medium rates of movement.

7. It is impossible to overcome great differences in the valuations which depend upon the individual observers, because the shoulder joints in the holding of the arm out, and moving of it allow the curves to vary too much on account of the radii and centres of movement.

B.

The Two-Handed Movements.

a. Influence of the Starting Points.

We consider, first, the differences in the individual series, which are due to changes in the heights of the starting points, in the lengths of the normal curves. In such psychological curves as these, where one investigates the ability to make two movements equal to one another, we have to deal with two groups of sensations rather than with two single sensations, as in the case of tone investigations, for example. For we can say that the two motions are equal to one

another when the two groups of sensations are equal. I give, first, the results of the changes in the heights of the starting points in influencing the valuation of these two groups. The heights of these remain for the normal curves in the same series always the same, while those of compared curves change with each series. In the upwards direction five groups were made, two by observer Kp., one with the right hand for the normal movements and one with the left hand for the same; two by O. (observer) S., and one by O. K. Each group had six or eight series and each of these is the average of ten experiments. In the diagrams 1, 2, 3, 4, both the upwards and downwards groups of movements are described.[1] Loeb (l. c.) has shown by his method that in upwards movements, where normal and compared movements are made simultaneously (simultaneous movements), the compared decrease more and more in comparison to the normal. They are undervalued. In the five groups considered now apart from changes in the lengths of the normal curves, there is only partly such a decrease. In groups c and h of O. S., such a decrease is most distinctly present. In group h the decrease from starting point (S. P.) —4 cm. to +6 is somewhat more than 2 cm. From S. P. +6 cm. up to S. P. +16, it is somewhat more than 1 cm. From starting point +16 up to +26 it increases a little, and then from +26 to +36 decreases nearly 2 cm. It is to be said here that in all these two-handed experiments the starting points of the normal hands were taken for observer Kp. at —40, for K. at —6, for R. at —10, and for S. at —4. In group c of the last there is an irregular increase from S. P. —24 up to S. P. +26, where a small increase at point +26, and then again a decrease up to +55, takes place. In the two groups, a and g, of O. Kp., the decrease is only partly to be met with. In group a, the line (Diag. 2)[1] sinks from S. P. —40, nearly 2 cm., down to —30. From —30 to —20 it rises 3.5 cm., and from —20 to —10, 1.50 cm. From —10 to 0.00 it decreases 3 cm., and at +10, .2 more. From +10 up to +20 a great deviation takes place and then it rises 2 cm. up to +30 cm. In this group, then, the increase and decrease in the differences vary. At first there is a decrease, then an increase, again a decrease and then again an increase. This variation is not dependent on the lengths of the normal curves, for the average of these does not vary sufficiently. Group b, of O. R. (Diag. 2)[1], shows an increase in the first three series and a decrease in the last three. This variation, with the increase in the heights of the starting points, agrees with group a of O. Kp., but agrees only partly with Loeb's results. In the other group, g, of O. Kp., there is an alteration between an increase and decrease, and *vice versa*, with each new starting point, and the results at the last starting point do not vary much from those at the first. The large variation in the lengths of the normal curves makes it possible that these last served as a means in making the valuations correct. We will later become acquainted with the influence which the lengths of the normal curves exert on the average differences. On the whole it is to be said that with upwards movements there is only partly an increase in the lengths of the compared curves in comparison with those of the normal where the starting points rise. It is worthy of remark that in group g, a, b and c, an inversely correct valuation takes place at the horizontal plane. From this point the average differences increase and decrease. Six groups of downwards motions were carried out, two by O. K., two by O. R. and two by O. S. In each case one was made with the right hand, for the normal curves, and one

[1] See original.

with the left for the same. In group d, of O. K., an increase in the average differences occurs from S. P. —6 up to +34. From here up to +64, a decrease takes place. Inversely, in group l of O. K., there is a decrease from —6 to +14 and an increase from +14 to +54. Loeb found that by his method an overvaluation took place with increase in heights of the starting points where the motion was downwards. He says, however, that these movements are not so correctly made as those in the opposite directions. In this group l of O. K., it is possible that the difference in the lengths of the normal curves from S. P. —6 to +14, makes a sufficient difference in the compared curves to explain the decrease. In group d there is also a difference in the normal curves in connection with the decrease in the starting points. I think, however, that this is more dependent on the heights of the starting points than on the differences in the lengths of the normal curves. We will later take the opportunity of treating this further. Group e of O. R. shows a regular increase from starting point —10 up to +30, which is possibly dependent upon changes in the lengths of the normal curves. Group m, by the same, shows five changes between increase and decrease from points —10 cm. up to +50. From +10 to +30 we find, with the exception of a decrease from 1.50 cm., between +10 and +20, a rather regular increase. From +30 to +40 it is possible that the difference between the lengths of the normal curves causes the decrease. There remain two groups of O. S. In group f we meet with a regular increase from —4 up to +26. From +26 up to +46 a decrease takes place, which is accompanied by a shortening of the normal curves. Between +46 and +56 an increase again is present. In group n a decrease, at first from —4 to +6, is seen, and after this a distinct increase from +6 up to +16. Between +16 and +26 a decrease occurs, and from +26 up to +56 the increase is clear enough. Probably the difference, 3.50 cm., in the lengths of the normal curves accounts somewhat for the small increase between points +26 and +36.

Apart, then, from the consideration of the differences caused by changes in the lengths and variations in the normal curves and by the time relations of the same, we can only agree partly with Dr. Loeb's results. Movements in both directions show variations from the increase and decrease in the differences which his method gives. These are greatest where the starting points for the compared movements are taken very high or very low. They also depend upon the observers.

b. Influences of the Lengths of the Normal Curves.

I give now some remarks upon the differences which are caused by the changes in the lengths of the normal curves. I treat the series each by itself, because otherwise the heights of the starting points would exercise too much influence. The individual series show the effect of the changes in the lengths of the normal curves without the variations which arise through these latter. Preliminarily we can say that changes in the lengths of the normal curves do influence the compared ones.

To show this I have used the M. R. V. The average of all the values obtained by this method for the upwards movements was 41.32, for the downwards 41.8. In the former, therefore, the individual compared curves decrease in comparison with their normal ones, with increase inthese latter. In the latter they rather increase than decrease. This increase is, however, rather small. One might say that in this case the normal and compared curves increase and

decrease relatively to one another, with increase in the lengths of the normal curves. This signifies that if a normal curve has a certain value, each increase of 1 cm. in the normal curves is accompanied by an increase of a little more than 1 cm. in the corresponding compared curves where the movements are downwards. Where these are upwards, the accompanying increase will be a little less than 1 cm. These increases and decreases, as determined by the method of relative valuation, vary according to the observer and the heights of the starting points. In group m of O. R., for example, this decrease rather increases with the elevating of the starting points. The average normal curves themselves do not vary much. In group e the average of the values shows an average increase, while in group m an average decrease. This agrees with the average differences. Again, in group l of O. K., a decrease obtained by this method shows itself from points —6 to +14, and from +14 to +54 an increase. This agrees with the decrease in the average differences from —6 to +14, and with the increase from +14 to +54. At starting point +14 the very high number, 96, is worthy of notice. In group d the values obtained by the M. R. V., from points +46 on, increase and decrease with the same of the average differences. Group n of O. S. shows at S. P. +36, and at +46, higher values, which accompany the increase in the average differences, and group f a high value, 80, corresponding to the greater increase in the average differences. The upwards movements of group g of O. Kp., apart from S. P. 0.00, show an agreement between the alterations in the average differences and in the values obtained by the M. R. V., so that an increase in the one accompanies that in another; so also with a decrease. From starting points —40 to —10, group a of O. Kp. agrees with the above, as also from S. P. 0.00 to +40. In group b of O. R. the increase and decrease obtained by this method follow the same in the average differences. In group h the values obtained by this method appear to show the opposite results to that of the increase and decrease in the average differences. From points +6 to +36 these values do not vary much, however, from such as are given by the differences when a relatively correct valuation takes place. In group c the values obtained by this method agree with the increase accompanying the elevating of the starting points of the average compared curves, in so far as both show an increase from —24 to —14 and under, from 0.00 to —56 a decrease. If we put these results now together, we find that the decrease and increase of the compared curves in each series, in comparison with their normal ones, agree sufficiently nearly with the decrease and increase in the average differences. From this it is to be assumed that the compared curves, in comparison with their normal ones in the separate series, vary according to the heights of the starting points and the direction of the motions. This presupposes the same for the differences between the average differences. The values obtained by the M. R. V. show in the downwards movements more of an increase than a decrease of the individual compared curves in comparison with their normal curves. This agrees with the average increase of the average compared curves over the average normal curves. On the contrary, a similar decrease is shown by the corresponding values of the upwards movements, and this agrees with the average decrease of the average compared curves over their normal curves. These values vary, also, as we have shown above, according to the heights of their starting points.

In both cases we find, however, that this decrease obtained by the M. R. V., in the case of the upwards movements and increase

in the case of the downwards, is not altogether complete. Were this so, then the values attained by this method for the decrease would be 0.00, and for the increase, 100. They are only relative values. In both cases the average values show deviations from the above. It is only the variations of these values which are large, according to the differences in the starting points, lengths of the normal curves and direction of the movements. It will still be our duty now to again consider the individual series and groups of series of the different observers in so far as all three changes mentioned above work together.

The Combined Effect of Changes in Heights of the Starting Points in the Lengths of the Normal Curves and in the Direction of Movement.

The average differences show,in general, with few exceptions, that at and above the horizontal point, the upwards movements exhibit a decrease of the average compared curves in comparison with their normal ones with the raising of the starting points, while the downwards ones show an increase. This is not very continuous in these experiments on account of the variations in the average normal curves and in the observers. After this general conclusion in regard to differences due to changes in the direction of movement, I consider now the combined effect of changes in the lengths of the normal curves and in the heights of the starting points.

(*a.*) In the downwards movements. The two groups of experiments of O. K. show that where the right hand makes the normal curve and the heights of the starting points are gradually increased from —6 to +34, and the left hand the normal curve with increase in the points from +14 to +54, the average compared curves in comparison with their normal curves more and more increase. In these groups the variations in the average normal lengths are small enough to leave them out of account. In group e of O. R., on the contrary, one can only compare the values from points +20 to +60 with one another. The variations of the average normal curves within these limits are not great. They show a distinct increase in the average differences with the increase in the average normal curves. In group m of O. R. the decrease in the average differences accompanying the raising of the points is dependent on this changing of the starting points. This occurs very exactly between points 0.00 and +30, and the variations in the average normal lengths between these points are very small. In the right handed group of O. S. there is a distinct increase in the average differences between the points —4 and +26, dependent on the changing between these points. From +26 to +46, the normal lengths vary too much to compare them with one another, while from points +46 to +56, there is again an increase in the average differences. In the left hand movements (where the left hand makes the normal curve) of O. S. there can only be considered an increase from points +36 up to +56. From the points +6 to +26 there is only a very small increase, and the variation of the average normal lengths between these points and between +36 and +56 is too great in order to compare the average differences between +6 and +56 with one another. In the downwards movements, then, apart from the left hand group of O. R., we find an increase in the average differences with the increase in the average normal lengths, where the variations in the average normal lengths in the groups are not large.

(*b.*) In the upwards movements. In group a of O. Kp. there is a decrease in the average differences between points —10 and +16 distinctly due to the changes in the starting points, because the

average normal lengths vary between these points very little. On the contrary between points —30 and —10 there is an increase. Between these points also, the average normal lengths vary very little. From these groups it is to be assumed that lowering of the starting points under the horizontal plane makes the average differences increase, but above the same, decrease. In group g of O. K. there is under the horizontal plane, —40 and —30, an increase, but above the same, between +10 and +20, a decrease. Apart from these groups the variations in the average normal curves are too great in order to compare the average normal differences with one another. In group h of O. S. there occurs between points +6 and +26 a distinct decrease in the average differences, which is dependent upon the raising of the starting points, because the variation of the average normal lengths is not great. Apart from these, there is in general a decrease, which, however, can be ascribed to the variation in the average normal lengths and not to that of the starting points. Group e of O. S. is in every way similar to group h. Group b of O. R. shows the average differences to decrease between +10 and +20 and between +30 and +40. Otherwise the variations in the average normal curves are too great in order to determine the influence of the starting points. In the upwards movements we have found that the average differences decrease with raising of the starting points. It would be possible, where this variation in the average normal lengths, above spoken of, is large in the groups, to reckon out a curve which would show the influence of the changes in the lengths of the average normal curves. In so far, however, as we have already found that, where these variations are not too great, in the upwards movements a decrease, of the compared curves in comparison with the normal ones, in the downwards ones, on the other hand, an increase, occurs, there is no purpose to be fulfilled. My experiments show, nevertheless, that the size of this increase and decrease is very much dependent on the observer. This can arise from the fact that the starting points of the normal curves bore different relations to the horizontal plane for the different observers. The normal starting points for observers Kp., K., R. and S. were respectively —40, —6, —10 and —4.

c. What kind of symmetrical results are given? There occurs a general tendency for the different compared curves to be made greater in comparison with their normal curves where the right hand is the normal than where the left hand is the same. This tendency varies with the observer and also with the height of the movement. The average of the average differences of the different series is reckoned out for each group. This average of a left hand group minus that of a right hand group gives the individual differences, which show the tendency above described. In groups d and l of observer K. this is very distinct. Here the left hand average is —3.05 cm., that of the right +3.23. The difference is 6.28 cm.; also in groups c and m of observer R. and in groups d and l of observer S., this tendency is clear. In the former two the average of the left hand motions is —.50 cm. and that of the right hand +3.93 cm., a difference of 4.13 cm. In the last two series the average of the left hand motions is —.19, that of the right hand +4.11, a difference of 4.30 cm. In the upwards movements in groups a and g of observer Kp., this tendency is not so distinct. The average of the left hand movements is —2.63, that of the right hand —1.21 cm., a difference of 1.42. In general, however, it is present especially at the beginning and end of the group. It is possible that the large variations in the lengths of the normal curves will explain this somewhat. In group a the limits between the largest and smallest

normal curves are roughly 13 and 22 cm. In group g they are 22 and 37 cm. In groups c and h of observer S. the general tendency is also present. The average of the average differences of the left hand movements is —1.17, that of the right +0.515 cm., a difference of 1.68 cm. In group b of observer R., we find no variation from the tendency in so far as the compared curves are less in extent than the same in the downwards group, which were right handed. The average of the former is +2.39 cm., that of group b +3.93 cm., and the difference is 1.54 cm. We have no corresponding group of the left hand upwards movements in order to compare it with group b.

The above mentioned figures of the M. R. V. show that from the standpoint of the relation of an increase in the individual normal curves, when one considers the series separately, to the making of their compared curves, a distinct asymmetrical phenomenon is present. Where the left hand makes the normal movements the compared curves decrease with increase in their normal curves more than where the right hand makes them. The M. R. V. yields values for the upwards movements of O. Kp., where they are left handed, of 36.5, and where they are right handed, 38.4. For O. S. the corresponding values are 38.4 and 45. Where the movements are in the downwards direction, the corresponding values for O. K. are 55 and 50. For O. R. the corresponding values are 42.7 and 59.5, and for O. S. 47.5 and 56.3. The single exception to this asymmetrical difference consists in O. K.'s case, where it is of the opposite nature.

Another asymmetrical difference is found in the averages of the average normal curves for the right and left hand groups along with the variations of these. While there was no strictness as to the making of the average normal curves in the series for the groups equal for the right and left handed experiments, still the variations in these show certain degrees of difference sensibility. The averages of the average normal curves in the downwards movements are for the observers R. K. and S., for the left hand, 15.00 cm., 27.17 and 12.42, and for the right hand, 11.14, 21.23 and 11.14 cm. respectively. It results, then, that the average of the average normal curves of each left hand group is greater than the same of each corresponding right hand one. Further, the averages of the average differences as we have mentioned above are similar to these. With each observer the average of the average differences of all the series of a right hand group is 4 to 6 cm. smaller than the same in a right hand group. This is so significant that an average undervaluation takes place where the groups are left handed, while an average overvaluation takes places in the right handed ones. The M. R. V. has shown that the compared curves in comparison with their normal curves decrease with increase in these last.

These three asymmetrical tendencies work together. The variations of the normal curves when a whole group comes into consideration give the decrease obtained by the M. R. V. an importance. If, *e. g.*, each increase in the normal curves of 1 cm. produced a difference of only +.04 cm. or +.05 in the difference between a normal and a compared curve, this could produce in 75 to 100 such, as occurs in a single group, a difference of 2.00 cm. to 3.00 cm. In some of the groups the average of the variations is 2 cm., and in all it is not less than 1.00. The differences between the limits of the largest and smallest normal curves of the groups themselves lie between 2.53 and 11.00 cm. There is here plenty of room for the decrease, as shown by the M. R. V. This decrease explains to some extent the undervaluation in the left hand groups and the overvaluation in the right hand. In the two downwards

groups of O. R. the averages decrease with raising of the starting points in the left hand movements. In right hand ones they increase. It can then scarcely be doubted that this difference between the average differences in the left and right hand groups is dependent on the corresponding results obtained by the decrease and increase in the values obtained by the M. R. V. With the other observers this difference is not so easy to explain in this way. We have referred to this above when we considered the influence of the changes in the starting points and in the normal curves upon the difference sensibility. It results, then, that in the downwards motions these three asymmetrical phenomena, namely, the average overvaluation of the right hand groups and undervaluation of the left hand, the greater extent of the average normal curves of the left hand than that of those of right hand, and the smaller values obtained by the M. R. V. in the left hand movements than in the right hand ones, accompany one another. Where the movements are in the upwards direction, the average of the average normal curves of the left hand group of O. Kp. is smaller than the same average of the right hand group, while by O. S. it is inverse. Here the average of the average normal curves of the left hand group is greater than the same of the right hand group, as we have found above with the downwards movements. In these upwards movements the difference between the average differences of the right and left hand groups is not at all so great as in the downwards ones. M. R. V. shows that a much greater decrease in the individual compared curves in comparison with their normal ones takes place in the right handed upwards movements than in the left handed.

d. The variations. I have so far considered the simple average difference without respect to the average variation, because the variations of the normal curves produce an effect upon the average variations of the different series of the groups. The average of all the average variations of the normal curves of the different series of O. Kp. is 1.991 cm. and of the compared curves 1.643 cm. The difference between the two is .348 cm. In the series of all the other observers the corresponding averages are smaller for the normal curves than for the compared. We call O. Kp.'s difference +.348 cm. The corresponding difference with O. K , is —.510 cm. The average of all the variations of his normal curves is 1.47 cm. and of his compared ones 1.98 cm. The corresponding averages of O. R. are 1.263 cm., and 1.47 producing a difference of —.206 cm. The corresponding averages of O. S. are 1.08 cm. and 1.25 cm. and the difference is —.17 cm. It appears now that the compared curves do not vary very much from the normal. What effect now do these variations of the normal and compared curves produce on the average variations of the series? Or of what importance are the latter? The average variation spoken of here was reckoned out as follows: The individual difference of the ten experiments give us +, — and 0 values. All the plus subtracted from all the minus give us an average difference, which can be +, — or 0. This average difference is now subtracted from the individual differences. The values so obtained are half plus and half minus when added together, and the average obtained by dividing by ten is the average variation of the series. The average of all these for O. Kp. is 1.84 cm.; for O. K., 2.04 cm.; for O. R., 1.363 cm. and for O. S., 1.38 cm. The average of the normal curves lies between 10 and 30 cm. In any case, then, these average variations are not very large. I have not reckoned out the differences between the average variations and the average differences in order to obtain from this the raw values for the sen-

sibility, because these variations are possibly too much influenced through the variations in the normal curves. Still I think that by further investigation these variations in connection with the average differences would give better values for the sensibility than the simple average differences. From the investigation, we have so far obtained no exact sensibility values for the different starting points. I think that the sensibility is very variable with the different observers,[1] so that one cannot obtain exact values for the difference sensibility, as has already been done in the other fields of psychology.

e. Influence of the metronome. In all these experiments the metronome beat sixty times in the minute. The duration of each motion could, then, equal one second. Where the two motions of each experiment were made together at the same time, they were made with each beat of the metronome. In case the compared motion followed the normal one, they were made so that the former followed the latter on the next beat, by O. K. and O. S., or the fourth beat, as by O. Kp. I find the great differences between the results of the variations of the normal and compared curves, which exist in the figures of O. Kp. and the other observers, to arise in this difference in the beating of the metronome. As above shown the average of all the variations of the normal curves is greater than the same of the compared ones by +.34 cm. In the results of the other observers it is smaller by +.29 cm. If the sensibility for space valuations from the standpoint of the different variations is very good, then the variations of the normal and compared curves would be equal. If the movements are made at the same time or immediately after one another, the variations of the normal and compared curves do not change. If the latter, however, accompany the fourth beat of the metronome, then their variations will be smaller because they are more influenced by the metronome, and the normal curves would also serve to give a better sensibility. The above described figures show this.

f. Influence of the weights. I have made forty-one series of experiments in order to estimate the influence of the weights upon the valuations of the motions. Twenty-three were made by O. S., twelve by O. R. and six by O. Kp. In the first twenty-three a weight of 1.31 kg. was fastened to the hand, which made the compared motions, and the starting points were varied, as in the above described experiments. Four groups of these are described on the diagrams X, R, j and K. The weight remained constant in these. In group x there is no great deviation from the ordinary group c as the normal lengths increase. The great deviation in group R is dependent on the time relations. In group R the compared motions were made at the same time as the normal ones. In group x they followed them. Group K agrees in general with group h. The curve for k stands only a little lower. Where the motions were downwards, group j gives a curve higher than group n. The results of groups R and j in comparison with group c agree with the results, which show the differences in the lengths of the normal curves. The groups 1, 2, 3, 4, 5 and 6, which are described to the left on diagram 2, were made by O. Kp. and O. R. with the left hand as the normal, and with weights of 0.5 kg., 1.31 kg. and 3 kgs. They were in the upwards direction. It is worthy of remark that all these groups of O. Kp. appear higher on the diagrams than his group a, which is to be taken into comparison with it. In groups 1 and 2 one observes an increase in the compared curves with the increase in the weight.

[1] "Der Muskelsinn der Blinder." Paul Hocheisen, Berlin.

In group 3 this is not continued. Groups 4, 5 and 6 of O. R. also show an increase in the lengths of the compared curves with the increase in the size of the weights, if one takes into consideration the increase in the lengths of the normal curves. These 3 groups appear lower (on the diagram) than group b of O. R. The groups 7, 8 and 9, diagram 3 of the downwards movements of O. R., with starting points of —2 cm., show an unsteady increase of the average differences, if the increase in the averages of the normal curves is considered. This increase is more doubtful in the groups where the points are —10 cm. In groups x and R of O. S., in groups j, 7, 8 and 9 of O. S. and O. R., respectively, and in groups 1, 2, 3, 4, 5 and 6 of O. Kp. and O. R., respectively, the average differences are greater where the motions were successive than those of the simultaneously made movements.

Results of the Pencil Method, Two-Hand Motions.

1. The results obtained by the M. R. V. show that where the normal and compared motions are considered in each series separately, an increase in the former is accompanied by a comparative decrease in the latter if the direction is upwards, if it is downwards by an increase. In both cases the starting points are above the horizontal plane, and the above results are more distinct the higher these points are raised.
2. With the raising of the starting points of the compared curves above the horizontal plane, the average differences increase in downwards movements, and decrease in upwards according to the observer and heights of the normal curves.
3. The unsteadiness as shown by comparing the average normal curves with one another by the different observers, makes it impossible to obtain the proper values for estimating the absolute or relative difference sensibility of these two-handed movements.
4. The movements where the right hand is the normal, exhibit an average overvaluation, and inversely where the left hand is the normal.
5. Where the left hand is the normal the average normal curves with equal effort of the will are greater than where the right hand is the normal.
6. The values obtained by the M. R. V. are, for left handed movements, less than those for right-handed.

The experiments with the weights show:—

7. Firstly, the average differences where a weight is used are larger than where the movements are made without such, in the case of downwards motions, and in the case of upwards motions smaller, apart from groups 1, 2 and 3 of O. K.
8. Secondly, the average differences of the successive movements are larger than those of the simultaneous.
9. Thirdly, the greater the weights are, the more the average differences decrease in upwards movements, and in downwards the reverse.

These experiments with the weights are, however, not sufficiently numerous, for the different observers, in order to lay much value on them.

II.

Experiments by the Angle Method, Using the Method of Just Perceptible Change.

General Description.

These experiments were made with Dr. Külpe, K.; Mr. Child, C.; Dr. Cohn, Ch.; Mr. Rogers, R., and the investigator himself, S., as observers. Eight series were obtained from O. K., four from O. C.,

four from O. Ch., two from O. R., and one from S. Some of the experiments of O. R. were intended to determine differences between valuations of active and passive movements. Those of the other observers were active motions with the horizontal plane as starting points as well as end points with motions whose starting points were above and below this. The rates were fixed by the beating of a metronome forty and one hundred and twenty times in the minute. The results obtained, where observer K. made the valuations, were in all the above directions and at all the different rates. The diagram[1] given shows the investigated extents of movement and the directions of the same. A is the centre of motion for the arm, BA is the horizontal plane, in which B is the starting point for downwards movements under this plane, and for upwards above it, and inversely the end point for upwards movements under, and for downwards movements above this. Downwards movements were made under and above this plane with normal distances of 15°, 30° and 60°, and with both rates. A few series of these motions were also made with 10° as normal extent under the 0 point (horizontal plane). The experiments of O. C. include the above described motions which lie under the 0 point, those of O. Ch. such as lie above it. I consider the experiments in the following order: those of each observer separately, those under the 0 point and afterwards those above it; at first the downwards experiments under the 0 point, then the upwards; the upwards above the same next, and lastly the downwards above it.

The two chief rates yield eight groups[1] of series for O. K., four for O. C. and four for O. Ch. In the tables the symbols[2] represent the following values: "r" is equal to the normal stimulus, r_o is the mean of the just greater than and the valuation of the extent above the same and just equal to it. "r"$_u$ is equal to the mean of the stimulus just smaller than the normal, and the valuation of the extent below the normal just equal to it. R is the mean of r_o and r_u. $\triangle r_o$ is equal to r_o-r, $\triangle r_u$ is equal to r-r_u. $\triangle r$ is equal to the mean of $\triangle r_o$ and $\triangle r_u$, while $\triangle$ is equal to R-r. Vr_o is the variation of the parts of r_o, and Vr_u that of those of r_u. Zr_o is the number of the parts of r_o, Zr of those of r_u. R shows the relative over and undervaluation. $\frac{\Delta r}{r}$ shows the approximate relative sensibility.

a. Under the Horizontal Plane.

Observer K. Group I. of O. K. consists of four series of downwards motions of extents of 10°, 15° and 30° under the horizontal plane, with a rate of forty beats of the metronome in the minute, and of one series of 60° at a rate of 120 beats a minute. We consider the first four series first. The variations in these vary between .37° and 1.00°. At 10° and 60° they are greater, while at 15° and 30° they are nearly alike. The valuating is, then, steadier at 15° and 30° than at 10° and 60°. The thresholds for difference sensibility, as shown by the values for $\triangle r$, which are .42°, .70° and .59°, increase from 15° to 30° and then decrease. This shows that the absolute difference sensibility does not increase regularly. The relative difference sensibility increases with the increase in the normal lengths, more between 10° and 15° and 30° and 60° than between 15° and 30°. At 15°, 30° and 60° the valuation is nearly correct. At 10° it is +.50°; at 15°, +.04°; and at

[1] See original.
[2] See Wundt's *Grundz. d. Phys. Psych.*

30° and 60°, —.05 and —.03°. The variations of all the series considered here of 10° normal extent can only be explained by the fact that the changes in the gradations of the compared extents was 1° instead of ½°. Still another series of 60° normal extent at the rate of twenty beats a minute shows an approximately correct valuation, namely, an undervaluation of .09°. The variations are greater than those at the more rapid rate. The threshold is .40°. The difference sensibility obtained by $\frac{\Delta r}{r}$ is finer than that of those of 60° at the more rapid rate.

Group II., of observer K., shows valuations of downwards movements at 10°, 15°, 30° and 60° under the horizontal plane at a rate of 120 beats in the minute. The variations of these vary for 10° and 30° between .68° and 1.25°, while for 15° and 60° they are respectively .30° and .63°. The individual valuations of these series are not so numerous as are those of the first. At 10° and 60° there is a large undervaluation, respectively .62° and .81°. At 15° one finds an undervaluation, of .12°, and at 30° an overvaluation of .19°. The thresholds, and along with them the absolute difference sensibility, increase with the increase in the normal extent. They are respectively for 15°, 30° and 60°, .41°, 1.05° and 1.31°. For 10° it is .87°. The relative difference sensibility is finest for 60°, less at 15°, still less at 30°, and still less again at 10°. From the standpoint of both thresholds and over and undervaluations, as also of the relative difference sensibility, the slower rate of motion is best in making downwards movements.

Group III. of O. K. was made in the upwards direction towards the horizontal plane at angles of 15°, 30° and 60°, and at rates of sixty beats of the metronome in the minute. The variations are, at 30° and 60°, more equal to one another than at 15°. Therefore, the steadiness is greater at 10° than at 30° and 60°. For angles of 10° and 60° overvaluations take place, and for 30° there is an undervaluation. The thresholds for 15°, 30° and 60° are respectively .34°, .48° and .65°. These signify an increase in the absolute difference sensibility with increase in the normal extents. The relative difference sensibility increases with the increase in the lengths of the normal angles (or curves, as they are seen on the table). The very small thresholds of this group are worthy of remark.

Group IV. of O. K. consists of upwards movements towards the horizontal plane at angles of 15°, 30° and 60°, and at a rate of 120 beats of the metronome in the minute. On the whole, undervaluations are manifest. These valuations at 15° and 60°, taking that at 30° as a standard, are the inverse of those in group III. In the last the angles at 30° are more overvalued than at 30° and 60°, while in group IV. the reverse is true. The beating of the metronome is the cause of this, which we shall consider later. At 30° the valuation is nearly correct, namely, —.07°. At 15° and 60° they are —.08° and —.13°. The thresholds are for 15°, 30° and 60°, respectively, .94°, .72° and 1.25°. They are also much greater than at rates of forty beats in the minute. Hence the absolute difference sensibilty increases regularly with the increase in the normal angles. On the other hand, the relative difference sensibility also increases with the same increase. The variation is at 30° smallest, at 60° larger, and at 15° greatest. They vary between 0.00° and .78°. We find now that the time relations have more influence upon the valuations than the directions. In the case of O. C. we will find that the direction exercises the greater influences.

Observer C. Group I. consists of downwards motions at 10°, 15°, 30° and 60° under the horizontal plane at a rate of forty beats of

the metronome in the minute. Vr_o and Vr_u vary between .50° and .90°. At 10° and 30° they are smallest, and at 15° and 60° largest. From the standpoint of the variations, it is to be assumed that the difference sensibility is smaller at 15° and 60° than at 10° and 30°. The threshold values ($\triangle r$) are for 10°, 15°, 30° and 60°, respectively, .87°, .76°, .87° and 1.16°. From 15° to 60°, then, the absolute difference sensibility decreases. At 10° it is the same as at 30°. R and $\triangle$ show an approximately equal valuation from the standpoint of the average valuation. At 10° there is a correct valuation. From 15° on the small undervaluation decreases up to 60°, where there is an overvaluation. $\frac{\Delta r}{r}$ shows the relative difference sensibility with increase in the normal angles to increase distinctly.

Group II. consists of downwards movements at 10°, 15°, 30° and 60° under the horizontal plane at a rate of 120 beats of the metronome in the minute. The variations in the values are smallest, namely, .50° and .75° at 10° and 30°, and largest at 15° and 60°, namely, 1.03° and 1.15°. At 10° there is a combined overvaluation of .25°, which increases to .50° at 15°, and at 30° again to .315°. The thresholds of 10° and 30° agree again with one another in this group. They are smaller than at 15° and 60°. At the first they are .75° and 1.09°, and at the last 1.25° and 1.37°. The absolute difference sensibility is, then, smaller at the first than at the last. $\frac{\Delta r}{r}$ shows that the relative difference sensibility decreases from 10° to 15° and increases from 15° to 60°. The difference between the relative sensibilities at 15° and 30° is greater than that between 30° and 60°. It results, then, that a slow rate of movement is better in valuing downwards movements than a more rapid rate.

Group III. consists of downwards movements at angles of 15°, 30° and 60° below, but towards the horizontal plane, at a rate of forty beats of the metronome in the minute. The number of individual valuations is, in the experiments that follow, less than formerly. At 15° there is an overvaluation of .26°, at 30° and 60° undervaluations of .21° and .13°. The thresholds increase gradually from 15° on. They are respectively .92°, 1.29° and 1.94° somewhat greater than in the downwards movements. The absolute difference sensibility decreases, then, with the increase in the normal extents. $\frac{\Delta r}{r}$ shows that the relative difference sensibility rather decreases between 15° and 30° and increases between 30° and 60°. The variations remain more constant in these groups than in the downwards movements.

Group IV. consists of downwards movements at angles of 15°, 30° and 60°, as in group III., at a rate of 120 beats in the minute. At 15° and 60° there are undervaluations of respectively .32° and .04°. At 30° an overvaluation of .34° is shown. The variations are also greater at 15° and 60° than at 30°. On this account there is a greater absolute difference sensibility at 30° than at 15° and 60°, from the standpoint of the variations. The thresholds increase in size from 15° on. They are .62°, 1.34° and 2.34° respectively for 15°, 30° and 60°. Groups III. and IV. show that the downwards movements are made more exactly from the standpoint of these than are the upwards. The relative difference sensibility obtained from $\frac{\Delta r}{r}$ does not agree with that obtained from the thresholds. At 60° it is finest, at 15° finer, and at 30° and 60° it is smallest.

One series was also made in the downwards direction by O. R. at 15° and at a rate of forty beats of the metronome in the minute. The valuation was very nearly correct, namely, an overvaluation of .04°. The accompanying variations were also small, .16° and .22°. The threshold agreed with that of the upwards movements. It was 1.52°. The small variations of the series of O. R. show that they are very valuable, although the thresholds are large.

I add here a series by O. S., which was made in the downwards direction below the horizontal point at angles of 30° and a rate of forty beats of the metronome a minute. An overvaluation of .37° took place, while the variations were .75° and .74°. The threshold and the relative difference sensibility agree with the experiments of O. C. and O. K. There is more of an overvaluation in these experiments than in the others.

b. Above the Horizontal Plane.

Observer K. The groups V., VI., VII. and VIII. of O. K. follow. They were made in the upwards and downwards directions above the horizontal plane at angles of 15°, 30° and 60° and at rates of forty and one hundred and twenty beats of the metronome in the minute.

Group V. consists of upwards movements at angles of 15°, 30° and 60° at rates of forty beats in the minute. The variations increase with the increase in the normal angles. $\triangle$ shows at 15° an overvaluation of .12° at 30°, an undervaluation of 16°, and at 60° one of .26°. The thresholds gradually increase with the increase in the normal angles. They are .37°, .59° and 1.12°. Hence the absolute difference sensibility decreases with the increase in the normal angle. $\frac{\Delta r}{r}$ shows a greater increase of the relative difference sensibility between 15° and 30° than between 30° and 60°.

Group VI. was similarly made to group V., apart from its more rapid rate of movement, which was 120 beats instead of forty in the minute. The variations increase with the increase in the normal angles. The valuations are the reverse of those in group V., in so far as the compared movements in comparison with the normal ones increase with the increase in these last. There is an undervaluation of .07° at 15°, one of .15° at 30° and of .56° at 60°. At 30° it is smaller than at 15° and 60°. The thresholds are: .56°, .40° and 1.19°, respectively, at 15°, 30° and 60°. At 30° it is somewhat smaller than at 15° and 60°. The absolute difference sensibility is smaller at the very large angles, greater at the medium ones and somewhat less at 60° than at 30°. The above mentioned differences between the valuation in series V. and VI. can only be explained through the change in the rate of movement.

Group VII. of O. K. consists of downwards movements from above towards the horizontal plane at angles of 15°, 30° and 60° and at rates of forty beats of the metronome in the minute. The variations increase here also with the increase in the normal angles. They vary between .16° and .75°. The valuations are related to one another similarly as in group V. At 15° there is an overvaluation valuation. The compared movements decrease, then, in comparison of .05° cm., at 30° an undervaluation of .10° and at 60° a correct with their normal ones with increase in these last. The thresholds are: .37°, .53° and .75° for 15°, 30° and 60° respectively. Hence the absolute difference sensibility decreases with increase in the normal angles. The relative difference sensibility increases more between 15° and 30° than between 30° and 60°.

Group VIII. of O. K. was different to group VII. in the rate of the metronome, which was 120 beats instead of forty, as in the latter. The variations increase here also with the increase in the normal angles. The valuations agree with those in group VI. At 15° and 30°, there are undervaluations of .04° and 1.19°, and at 60° an overvaluation of .27°. In connection with the increase of compared angles as compared with that of normal ones, groups V. and VI. agree with groups VII. and VIII. This agreement corresponds with the variations in the rate of movement. The thresholds show here also an increase in the absolute difference sensibility with increase in the normal angles. They are respectively .40°, .56° and 1.09° for angles 15°, 30° and 60°. The relative difference sensibility increases gradually with the increase in the normal angles. Groups VII. and VIII. show that the valuation is more exact at the rates of forty beats of the metronome than at the rates of 120, and this from three standpoints, namely, that of the valuation as determined by $\triangle$, that of the absolute difference sensibility, and that of the relative difference sensibility determined by $\frac{\Delta r}{r}$.

Observer Ch. Groups I., II., III. and IV. of O. Ch. were carried out similarly to groups VI., VII., VIII. and IX. of O. K. Group I. consists of upwards movements, etc. The variations are in this group especially large compared with the same in the groups so far considered. They vary between 1.02° and 2.66°, and are much larger at 60° than at 15° and 30°. The valuations are undervaluations. At 15° and 30°, they are .43° and .09°, the latter an approximately correct valuation. The compared angles at 15° and 60° are more undervalued in comparison with their normal angles than at 30°. The threshold at 30° is smaller than at 60°, and at 60° smaller than at 15°. The absolute difference sensibility follows this order also. The thresholds are for 15°, 30° and 60°, respectively, 1.14°, .76° and .85°. The relative difference sensibility increases more between 15° and 30° than between 30° and 60°.

Group II. of O. Ch. varies from group I. only in so far as the rate is 120 instead of forty beats per minute. The variations vary between .44° and .75°, and are somewhat greater at 15° and 30° than at 60°. The valuations are at 15° and 60° overvaluations, namely, .26° and 1.85°, respectively. At 30° there is an undervaluation of .18°. The relation of changes of the compared angles to their normal ones is the reverse of what they were in group I., where the normal angles vary. The thresholds are greater than in all the groups considered above. They are for 15°, 30° and 60°, respectively, 1.48°, 1.71° and 3.15°. Hence the absolute difference sensibility decreases with the increase in the normal angles. The relative difference sensibility increases much more between 15° and 30° than between 30° and 60°.

Group III. of O. Ch. consists of downwards movements at angles of 15°, 30° and 60° at a rate of forty beats of the metronome in the minute. The variations follow in their size those of the normal series. At 30° there is an overvaluation of .06°. At 15° and 60° there is an undervaluation of .07°, and an overvaluation of 1.25° respectively. The valuations at 15° and 30° are nearly correct valuations. The thresholds do not vary relatively as do the normal angles. At 15°, 30° and 60° they are respectively 1.00°, .93° and 1.47°, while the absolute difference sensibility is smaller at 60° than at 15°. This last is somewhat less than at 30°. On the contrary, the relative difference sensibility is finer at 60° than at 30°, and finer at 30° than at 15°. The difference between these two is, however, nineteen times greater than that between the first two.

Group IV. has normal angles of 15°, 30° and 60°. The movements were quite similar to those in the above group apart from a change in the rate of forty beats of the metronome to that of 120. $\triangle$ shows an overvaluation of .21° and .37° at 15° and 60°, and an undervaluation of .19° at 30°. $\triangle r$ gives thresholds of 1.20°, 1.00° and 3.00° at 15°, 30° and 60°, and $\frac{\Delta r}{r}$ shows a higher relative difference sensibility at 30° than at 15° and 60°. The relative difference sensibility is finer at 30° in group III. than in group IV.

Valuations of Passive as Compared with Active Movements.

With O. R. passive movements, as well as active, for the purpose of comparing them with the active, were made in the upwards direction under but towards the horizontal plane. The normal angles were 15° and the rates forty (group I.) and 120 (group II.) beats of the metronome in the minute. The variations of both groups are not large. In group I. they vary between .32° and .44°. In group II. between .29° and .69° $\triangle$ shows that the valuations on the whole are finer with the passive than with the active movements. In the first there was an overvaluation of only .05° for group I. and of .03° for group II., while for the last there were undervaluations of .23° and .36°. The thresholds in group II. are nearly equal for the active and passive movements, namely, 1.48° and 1.41°. Those in group I. are not nearly so much like one another, since the values are 1.40° and 1.85°. The absolute difference sensibility is finer at the slower rate for the active than for the passive series, and for the quicker rate than for the opposite. The constant size of the thresholds with O. R. are probably dependent on the want of practice, or on his want of sensibility for motion sensations. The relative difference sensibility yields like results to those of the absolute. While now we have so far described the differences between active and passive motions, yet we can now leave them aside, as they are so limited. They serve only to show influences of the rate of movement.

The Relations of Changes in the Rate of Movement.

So far we have considered these experiments on the whole without respect to changes in the rates, which arise within the groups themselves. All the possible rates to be found are as follows: 15° in 1.50 sec. = 1° in .10 sec., 15° in .50 sec. = 1° in .033 sec., 30° in 1.50 sec. = 1° in .05 sec., 30° in .50 sec. = 1° in .016 sec., 60° in 1.50 sec. = 1° in .025 sec., 60° in .50 sec. = 1° in .008 sec., 60° in 3.00 sec. = 1° in .05 sec. If the metronome beats forty times in the minute, each interval is equal to 1.50 sec.; if 120, .50 sec., and if twenty times, 3.00 sec. Applying these figures to the groups we have described, we find that in each series the rate increases at 15°, 30° and 60° in a geometrical progression of one-half. One group of motions with 15°, 30° and 60° as normal angles and a rate of forty beats in the minute, implies relative rates of for 15°, 30° and 60°, 1° in .100 sec., 1° in .05 sec. and 1° in .025. Now these increase in the relation of a geometrical progression of one-half to one another. Where the metronome beats 120 times in the minute, the corresponding relative rates are 1° in .03 sec., 1° in .016 sec., 1° in .008 sec. These stand to one another in the relation of a geometrical progression of one-half. I presuppose here that if the metronome beats forty times a minute, the movements last the length of time between the two beats. If this is not exactly so, still the variations from this at

15°, 30° and 60° is nearly alike in each case and very small, and moreover these relative rates show the exact duration of the movements.

On the Relation between Stimuli and Difference Sensibility.

The sizes of the normal angles themselves bear to one another the relation of a geometrical progression of one-half, and the rates also bear the same relation to one another. This is true, also, although the duration of movements for the different normal angles remains the same. If we consider the size of these angles, with the rates of movement as stimuli, then we have in each group, considered above, three stimuli, which bear to one another the relations of parts of a geometrical progression. Now, Weber's law says that in order to let the rate of movement increase in an arithmetical progression, the stimuli must increase in a geometrical progression. Of the eight groups of O. K. there are only four where this law holds approximately, namely, in groups III., IV., VII. and VIII. In all the groups the direction is towards the horizontal plane; III. and IV. upwards, from below, towards the horizontal plane; VII. and VIII. downwards from above, towards the same. As measure of the sensibility I have taken $\frac{\triangle r}{r}$. When one reckons out this sensibility value to the tens of thousands, it is sufficiently approximate without reckoning out $\frac{\Delta r_o + \Delta r_x}{2}$. The values obtained by this last vary only five to ten thousand parts from those obtained by the $\frac{\Delta r}{r}$. In group III., O. K., the difference sensibility for 15°, 30° and 60° respectively, is represented by 226, 160 and 108. The differences are, therefore, sufficiently approximate to an arithmetical mean of 60. In group IV. the arithmetical mean is about 57, in group VII. about 60, in group VIII. about 65. In these four groups Weber's law appears to hold. In the other four groups of O. K. the changes in sensibility do not follow any distinct law. In group I. the sensibility at 60° is nearly three times finer than at 15°, while the difference between that at 30° and 60° is nearly four times greater than that of the same between 15° and 30°. This difference might easily come about through habitual movements in this direction and practice. In group II. the sensibility is in general not great. That it is greater at 15° than at 30° can be explained through habit. In group V. the small sensibility at 60° prevents Weber's law from being applicable to the results. These motions at 60° are uncustomary. In group VI. the difference sensibility is finer at 30° than at 15° and 60°. I see no explanation of the variations in the sensibility in this group. In the ordinary movements which one makes, the direction is towards the horizontal plane. Goldscheider[1] has shown that sensations of locality exist by which one can recognize the place where the arm is held. In the ordinary movements towards the horizontal plane, these sensations of locality are better developed. The more regular sensibility for different distances in motion towards this plane can be influenced through this. Only in group III. of O. C. is there any approximate application of Weber's law. The difference between the values for sensibility at 15° and 30° is 183, between those at 30° and 60°, 107, an approximate arithmetical mean of 145. In group IV. the sensibility is throughout not fine. That at 15° appears, however, to be finer than that of the same

[1] "Ueber den Muskelsinn." Du Bois-Reymond's Archiv, 1889 and Appendix.

observer for other directions and rates of movement. The very small sensibility which O. C. shows in comparison with O. K. throughout is worthy of attention. The differences between the sensibilities at 15° and 30° and 30° and 60°, in groups I. and II. of O. C., are peculiar, in so far as in both cases the difference between the first two is much finer than that between the last two. Between 30° and 60°, in group I., the sensibility increases 2.29 times more than between 15° and 30°, and in group II. 3.48, times. With O. Ch. Weber's law does not hold at all. In group I. the difference between 15° and 30° is 4.5 greater than that between 30° and 60°, and in group II. nine times. It appears in groups I. and II. of O. C. and O. Ch., that while Weber's law does not hold, a law of another kind holds. This appears to be dependent on the rate of movement in so far as the tripling of the rate of movement doubles the relation of the differences in the sensibility of either of the observers. In the case of the first observer 9 is twice as great as 4.5, and in that of the last, 3.48 is 1½ times as great as 2.26. This doubling of the relations of the differences to one another is accompanied by a triple increase in the rates of movement for both observers. For 15°, 30° and 60° the rates are respectively—in group I., 1° in .10 sec., 1° in .05 sec. and 1° in .025 sec., and in group II., 1° in .033 sec., 1° in .016 sec. and 1° in .008 sec. The groups of O. K., where the deviations from Weber's law are found, namely, I., II., IV. and VI., correspond to these groups. The differences in these groups agree, however, only partly with the results of O. C. and O. Ch. In group III. 4 is twice as great as 2 in group I. (the relative values for the relation of the differences). Group VI., however, yields no value to be compared with 4 in group V. Series III. and IV. of O. Ch. allow of no application of Weber's law which the corresponding groups of O. K. show. The very fine difference sensibility at 30°, in comparison with that at 15° and 60°, explains this partly. It appears, then, that movements towards the horizontal plane, as, *e. g.*, III., IV., VII. and VIII of O. R., III. and IV. of O. C., and III. and IV. of O. Ch., with exceptions, follow Weber's law, while such as have a direction of movement away from the horizontal plane, as, *e. g.*, groups I., II., V. and VI. of O. K., I. and II. of O. C. and O. Ch., give greater differences of difference sensibility between 15° and 30° and 30° and 60° than Weber's law presupposes. The peculiarities in the different kinds of movements, then, are dependent as well on the normal lengths as on the rates of movement, and these latter are probably influenced by the direction of movement.

The Relations of the Valuation to One Another.

The table given on page 402 shows the values for $\triangle$, for the different directions and groups of O. K., O. C. and O. Ch. I. to IV. show it for motions which were made under the horizontal plane, and V. to VIII. for such as were above the same, a and b represent rates of respectively forty and 120 beats of the metronome in the minute. U and D show whether the movements were up or down. The symbols minus and plus, when they are enclosed in brackets, show whether there was an over or undervaluation. When the plus and minus stand outside of these, they show the relative valuations of the three normal angles, 15°, 30° and 60°, to one another. The values for the three normal angles are given from left to right for angles of 15°, 30° and 60° in each diagram. I., III., V. and VII. are upwards movements, II., IV., VI. and VIII. downwards.

TABLE III.

U.					D.				
		15°	30°	60°			15°	30°	60°
I C.	a	+(+.26)	−(−.21)	+(−.13)	II. C.)	a	−(−.05)	−(−.04)	+(−.03)
	b	−(−.32)	+(+.34)	−(−.04)		b	+(+.50)	+(+.34)	−(−.31)
III. K.	a	+(+.02	−(−.27)	+(+.27)	IV. K.	a	+(−.04)	−(−.05)	+(−.03)
	b	−(−.68)	+(−.07	−(−.13)		b	−(−.12)	+(+.19)	−(−.81)
V. Ch.	a	−(−.43)	+(−.09	−(−.43)	VI. Ch.	a	−(−.07)	+(+.06)	+(+1.25)
	b	+(+.26)	−(−.18)	+(+.35)		b	+(+.21)	−(−.19)	+(+.37)
VII. K.	a	+(+.12)	−(−.16)	−(−.26)	VIII. K.	a	+(+.05)	−(−.10)	+(0.00)
	b	−(.07)	+(+.15)	+(+.56)		b	+(−.04)	−(−.19)	+(+.27)

It results that the best valuation for downwards movements was at the rate of forty beats a minute, namely, in groups a II., a IV., a VI. and a VII. There is almost a correct valuation by each observer for all the angles. A single exception to this is at 60°, by O. Ch. The valuations of the other movements vary between an overvaluation of .56° and an undervaluation of .81°. In only b III. and a V. is there an undervaluation for all three normal angles. In all the other groups there are more or less overvaluations as well as undervaluations. In no group are there undervaluations alone. Apart from groups b III. and a V. it is, then, to be assumed that if movements had been made at the angles lying between the angles used as normals, with similar rates of movement (forty or 120 beats), correct valuations would have resulted. One finds, further, from the tables, with few exceptions, an overvaluation where the metronome beats forty times a minute, and inversely an undervaluation where it beats 120 a minute for normal angles of 15° and 60°. At 30° the reverse holds true. From this it is further to be assumed that if one investigated the angles between the normal angles with both rates of movement, several such normal angles between 15° and 60° would be found for which correct valuations would be given.

That at the two rates of movement for the same normal angles and movements, the relative valuations are the inverse to one another, is shown in the tables by the plus and minus symbols marked outside of the brackets. The relations of the over and undervaluations and time of the changes in the valuations are not the same in all the groups. In the first groups, namely, for motions under the horizontal plane, there is for angles 15° and 60°, and at the slow rates of movement, a relative overvaluation, and at 30° a relative undervaluation, at the quicker rates of movement for 15° and 60° inversely a relative undervaluation, and at 30° a relative overvaluation. Group II. shows some exceptions to this. The relative over and undervaluation signify nothing else than that in one case the angles are more highly valued at 15° and 60° than at 30°; in the other case, less. Group II. shows that the smaller angles at the slower rate are less valued than the larger ones. When the rate is quicker the reverse holds. When the motions were made above the horizontal plane, the relations of the valuation change alternately with the groups. Still the rates of movement show

their peculiar inverse changes in the valuations. In groups V. and VI. the angles are less valued than at 30°, where the slow rate of movement is used. Where the quick rate is used the reverse is true. In group VII., at the slow rate, the smaller angles are higher valued than are the larger, and at the quicker rate the opposite is true. Group VIII. shows that the angles at 15° and 60° are higher valued at the slow rate of movement than those at 30°, and at the quicker rate less valued at 15° and 60° than at 30°.

The Steadiness of the Motions.

The downwards movements appear on the whole to be valued with less certainty than the upwards. The average of all the variations of the groups of the latter for O. C. is .62°, and the same of the downwards .795°. The average of all the former of O. K. under the horizontal plane is .4009°, the same of the latter .684°. Above the horizontal plane the corresponding figures are .35° and .40°. The groups of O. Ch. give an opposite result in so far as the average of the variations of the upwards motions is 1.14° and that of the downwards .53°. It is worthy of remark that the upwards movements of this observer were less exact where the rate was slow than where it was quick, as the variations of the former ranged between 1.02° and 2.66°, and those of the latter between .44° and .75°. In so far as the average of the variations was .46° at the slower rate and .59 at the quicker of the downwards movements, the reverse holds true in regard to them. This observer could only with difficulty execute the larger normal angles at the quicker rate for experimental purposes, and his motions appear more as reflexes than as regulated movements.

TABLE IV.

U.					D.				
		15°	30°	60°			15°	30°	60°
I. C.	a	613 +(+)	430 −(−)	323 +(−)	II. C.	a	512 −(−)	291 −(−)	194 +(+)
	b	416 −(−)	446 +(+)	393 −(−)		b	833 +(+)	365 +(+)	229 −(−)
III. K.	a	226 +(+)	160 −(−)	108 +(+)	IV. K.	a	280 +(+)	233 −(−)	99 +(−)
	b	283 −(−)	240 +(−)	208 −(−)		b	280 −(−)	351 +(+)	219 −(−)
V. Ch.	a	760 −(−)	253 +(−)	141 −(−)	VI. Ch.	a	666 −(−)	145 +(+)	291 +(+)
	b	986 +(+)	570 −(−)	525 +(+)		b	806 +(+)	333 −(−)	500 +(+)
VII. K.	a	246 +(+)	196 −(−)	186 −(−)	VIII. K.	a	246 +(+)	176 −(−)	125 +(0.00)
	b	373 −(−)	733 +(+)	198 +(+)		b	266 +(−)	186 −(−)	136 +(+)

In tables similar to the above I have put in the values for the relative difference sensibility. In groups II., III., VI., VIII. and V., and for the most part also in groups I. and VII., it appears that the

movements are executed with finer difference sensibility at the slow rate than at the quick. In so far as the relatively smaller rate in any group accompanies the greater angles, it would be desirable to complete these groups so that all the normal angles would have the same rate of movement.

I have completed two other groups below the horizontal plane—one upwards, group II., and one downwards group I.,—in which the rate was kept relatively the same for 15°, 30° and 60°, namely 1° in .033 seconds. For this purpose the metronome beat at 15° 120 times a minute, at 30° sixty times and at 60° thirty times. The O. Hicks was at first somewhat exercised, and afterwards four series of upwards and four series of downwards movements in the order of experimentation were varied alternately with one another. Those for the purpose of exercise were in the downwards direction, and consisted in eight series at respectively 15°, 30° and 60°. These eight series I have reckoned out with the last four similar series, because both the four series and the combined twelve series show similar deviations from the four series of upwards movements.

A very distinct difference between the results of groups I. and II. is present, in so far as $\triangle$ shows that a general overvaluation of the upwards movements and undervaluation of the downwards take place. From this and from the other observers it is to be assumed that the direction of the valuation is dependent on the observer. In group I. $\triangle r$ and $\frac{\Delta r}{r}$ show that increasing the size of the normal angles causes the absolute difference sensibility to decrease and the relative to increase. $\triangle$ shows also that this increase also causes the valuation to be more correct. The variations varied between .28° and .54° and gradually increased with the increase in the normal angles. The difference between the relative difference sensibility, as shown by $\frac{\Delta r}{r}$ at 15° and 30°, is 1.5 times that shown at 30° and 60°.

Group II. of O. H. is the reverse of group I. in so far as $\triangle r$ and $\frac{\triangle r}{r}$ show that increasing the angles decreases the absolute difference sensibility, but increases the relative. This holds for angles of 15° and 60°. At 30°, however, the absolute difference sensibility, as shown by $\frac{\triangle r}{r}$, is finer than at 60°. The values of $\triangle$ do not vary much from one another. The variations Vr_o and Vr_u vary between .12° and .62° and increase with the increase in the normal lengths. The relative difference sensibility is much smaller at 15° than at 30° and 60°. Both series I. and II. show, then, that the increase in the rates with increase in the normal angles produces no great influence upon the increases in the relative sensibility as shown by $\frac{\triangle r}{r}$.

Sense of Locality.

I have called attention to sensations of locality for the arm. O. C. has observed this further in so far as in his upwards movements the starting points instead of the end points were varied. He recognized the changing of the angles in a series by the change in the height to which he must raise or lower his arm in order to reach the starting points. One series of group VII. of O. K. was executed in which the starting point instead of the end point was varied. The valuation and the threshold values are about the same as in

the regularly executed series. From this it appears that the difference in the valuation is not large. It appears, nevertheless, that here an apparently fruitful field for investigation on the sense of locality[1] by the method of just perceptible change, is open. If a height is taken as normal starting point and the thresholds above and below determined, values of the difference sensibility in the sense of locality would be obtained. The values of the average of the heights of the starting points in my first experiments could also be very easily used here, in so far as they show the average variations of the individual valuations of the heights of any assumed starting point.

The results of these angle method experiments by the method of just perceptible change are:

1. The absolute difference sensibility as determined by $\triangle r$ for 15°, 30°, 60° is greater at the slower rate of forty beats of the metronome a minute than at the quicker rate of 120 beats.
2. This absolute difference sensibility varies with the observers and with the size of the normal angles.
3. At 30° and 60° it is with few exceptions smaller than at 15°.
4. The relative difference sensibility as shown by $\frac{\Delta r}{r}$ is much finer at the larger normal angles. For motions which are directed towards the horizontal plane, Weber's law holds approximately. For such as are directed away from it, the increase in the relative difference sensibility with increase in the normal angles is much greater than shown by Weber's law.
5. By proper use of different rates it is possible for different observers to execute correctly, according to the values given by $\triangle$, movements at all normal lengths.
6. The absolute difference sensibility as shown by $\triangle r$ is finer at quicker rates of movement than that at slower, where the motions are upwards towards the horizontal plane.
7. The difference between the thresholds for motions above and under the horizontal plane is not large for the same observer.
8. Where changes in the size of the normal angles and in the rates take place, the changes in the first produce the greatest influence.
9. The steadiness in the valuations is dependent on the observer. In the case of most observers, valuations of downwards movements are less steady than are those of upwards.

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[1] Goldscheider above cited.

TABLE I.—*Downwards One-Handed Movements.*

N. S.	N. E.	H. S.[2]	H. V.	M. R. V.	N. L.	V. N. L.	M. D.	V. D.	R. L.	V. R. L.	M. D. R. L.	V. D R.	O.
1	9	—53.95	1.80	30	15.97	2.15	1.05	2.95	46.81	2.86	4.50		S.
2	12	—50.00	50.—58.	32	10.02	.86	3.48						S.
3	14	—47.00	2.00		11.30	1.45	.307						S.
4	15	—46.00	84.—59.		16.95	3.48	4.03						K.
5	4	—42.50	2.00	0.00	12.93	2.33	5.87	.68	55.00	8.50	—2.50	13.50	S.
6	17	—32.65	1.68	31½	21.98	1.40	—4.83	2.36	60.77	11.75	5.03	14.56	S.
7	10	—09.00	3.94		18.67	3.68	1.47						K.
8[1]	15	—00.00	+14 +30		10.41	1.43	2.28						K.
9	14	+14.50	3.00		36.06	2.95	—.41						K.
10	5	+22.46	3.94		13.00	1.70	4.40						K.
11	71	+41.76	1.82	30	28.45	3.09	.77	3.38	67.50	6.54	—6.18	12.25	S.
12	9	+46.67	3.26	25	37.61	3.42	3.00						S.
13	8	+49.98	4.00	00	34.47	5.02	—1.63						S.
14	8	+56.07	4.81	46	16.62	2.22	2.28						W.
15	24	+58.59	1.20		24.82	2.67	8.85						K.
16	48	+63.00	4.00		59.98	9.74	3.45						S.
17	30	+63.50	4.01		27.97	11.67	2.86						K.
18	23	+70.00	2.00—3.00—		9.98	.91	.57						S.
19	12	+70.37	.85	59	12.41	1.40	4.14						S.
20	15	+72.00	+72 —22	85¼	6.75	.70	1.21						K.
21[2]	10	+50.00			19.42	3.07	1.82						S.

[1]Observer sat. [2]Made by shadow method.

N. S., number of the series. N. E., number of experiments in each series. H. S., average height of starting point for each series. H. V., average variation of same. M. R. V., as described above. N. L., average length of normal curves of each series. V. N. L., average variation of same. M. D., average difference between normal and compared curves of each series. V. D., average variation of same. R. L., average length of radii of normal curves. V. R. L., average variation of same. M. D. R. L., average difference between radii of normal and compared curves. V. D. R., average variation of same. O., observer.

TABLE II.—*Upwards One-Handed Movements.*

TABLE II.—*Upwards One-Handed Movements.*

N. S.	N. E.	H. S.[2]	H. V.	M. R. V.	N. L.	V. N. L.	M. D.	V. D.	R. L.	V. R. L.	M. D. R. L.	V. D. R.	O.
1	9	—59.28	1.15	90	21.25	1.88	6.77						S.
2	18	—58.73	.91	52	19.83	4.35	8.29	4.35	47.33	?	7.90	8.55	S.
3	15	—55.00	38.72	38¼	13.95	2.05	6.66						S.
4	15	—54.97	2.33	33	16.93	1.73	3.30						S.
5	12	—54.67	1.24		19.54	2.38	2.54	2.18	50.91	3.83	9.75	4.79	S.
6	10	—53.70	1.80	48	16.50	1.55	4.50	2.10	59.10	12.28	10.50	16.44	S.
7	15	—51.17	1.83	45¼	35.45	4.68	9.80	3.46	57.66	5.39	2.83	6.20	S.
8	15	—48.34	1.37	39	14.15	1.92	3.91						S.
9	17	—41.92	2.39	55	41.96	6.28	12.10	7.46	68.58	6.32	4.94	8.19	S.
10	8	—36.63	3.62	18¾	36.68	4.29	6.68						S.
11[1]	15	—34.00	3.72		16.91	1.85	4.21						K.
12[1]	11	—33.00	2.26		10.81	2.30	.51						K.
13	23	—32.20	1.70	93	29.73	1.69	13.91						S.
14	40	—23.59	3.04		14.10	1.53	2.93						S.
15	8	—22.25	4.31	43¾	32.53	2.29	1.81						S.
16	4	—16.13	3.31	75	10.68	1.95	2.31						S.
17	12	—15.70	3.71	54	26.33	2.22	4.86	2.46	63.75	9.21	.87	10.45	S.
18	31	—12.10	4.25		14.27	1.92	5.43						S.
19	15	—10.20	2.13	45½	48.03	5.17	4.65						K.
20	22	— 6.30	4.13		33.33	6.17	5.80						S.
21	14	— 4.93	3.14	71½	20.78	1.69	10.18						S.
22	15	+14.25	2.68	30	23.75	1.42	4.89						S.
23	16	+15.60	3.19	51	19.54	3.53	5.31						S.
24	13	+17.35	2.12	21½	13.82	1.54	6.19						S.
25	15	+18.33	2.03	61½	14.78	2.47	4.33						K.
26	15	+19.24	2.51	30	15.58	2.10	3.89						S.
27	13	+31.79	1.80	63	15.15	1.69	5.10	2.43	37.54	7.03	9.03	8.15	S.
28	20	+42.00	20.—64.		7.63	1.44	1.00						K.
29	14	+56.79	1.28	44	11.19	1.18	6.07						S.
30	15	+70.00	3.00—5.00		12.20	1.45	.35						S.

[1]Observer sat.

[2]Roughly estimated.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY.

Communicated by E. B. TITCHENER.

III.

TWO POINTS IN REACTION-TIME EXPERIMENTATION.

By R. WATANABE, PH. M.

We suppose that every psychologist hopes to win something for *psychology* from experiments made upon the simple reaction-time. The simple reaction is the exact type of a voluntary action, and its study should throw light upon the processes involved in this. At present, however, but little has been done towards a psychological interpretation of reaction results, despite the great extent of reaction-literature and the mass of published figures. Indeed, Lange's distinction of the sensorial and muscular forms—which has aroused so much controversy—and Martius' supplementing of it by the introduction of a third (central) form, have constituted almost exclusively the basis upon which psychological discussion has rested.

In the present paper, two questions will be treated: that of the value of subjective (introspective) control of the simple reaction-experiment, and that of the canceling-out of refractory figures. We shall endeavor to deal with each from the psychological point of view.

(1) What is the value of introspective control of the duration of a reaction? Martius ascribes a considerable degree of importance to it, though admitting that introspection is not adequate to the exact estimation of *Störungswerthe*.[1] But the question is not that of whether introspection can determine the relative durations of reactions of 129 σ and 130 σ—to take an extreme instance from Martius[2]—but rather this: can self-observation place a given experiment under the rubrics, "conditions good, instructions followed," and "conditions bad," or "instructions not followed," with such accuracy, that its verdict suffices at once to admit the experimental result to, or exclude it from the durations employed by the experimenter in averaging? The mean variation of the simple sensorial reaction, *e. g.*, may normally amount, in certain sense departments, to 40σ. Does self-observation reject times, which, if included in the final calculation, would raise the mean variation above that normal magnitude? We wish again to emphasize the fact that it is for this question a matter of indifference whether

[1] Uber die muskuläre Reaction und die Aufmerksamkeit. *Phil. Stud.*, VI. pp. 196-7; 204-5; 207; 209-10; 214; 216. Dr. Bliss curiously makes Martius' results entirely negative. *Studies from the Yale Psychological Laboratory*, 1892-3, p. 36.

[2] P. 204.

we can exactly estimate time-periods of 100-300 σ, or not. We possess now our objective time-norms; we can check our own experiments by reference to those norms; we can pass from the investigation of the "time" of reaction to further investigation. It is, therefore, not enough to state—or to prove—that "the mind is unable to accurately estimate small divisions of time under the most favorable conditions."[1] The "mind" works through the *Reproduktion der Theilelemente des einzelnen Reactionsvorgangs, soweit er psychischer Natur ist.*[2] So that—to restate once more—the problem runs: is this reproduction sufficiently accurate to serve as a control of the admission or rejection of the single experimental result in the experimental tables?

Dr. Bliss finds that introspection is "of great value in estimating the general conditions of an experiment and showing the influences which affect the results," though it is "not to be trusted in estimating the results themselves,"[3] since our time-estimation is not sufficiently accurate. It is clear, we think, from what has been said above, that the writer has misapprehended the function and manner-of-working of introspection in this particular case. He continues: "Its (the mind's) judgment is particularly liable to be affected by the conditions of the reaction. Its report is what it thinks ought to be, rather than what it actually sees." But this is merely a charging of the "mind" with liability to autosuggestion and *Einstellung;*[4] and these are "constant errors" in all psychological experimentation, the end of which is the recording of a judgment.[5] So far, therefore, a clear case against introspection has hardly been made out.[6]

The issue can only be decided by appeal to experimentation. And we believe that, here as elsewhere, strict regard must be had to the sensorial muscular difference. Experiments which were carried out at the Leipzig Institute,[7] and others which have been made at Cornell,[8] point, as it seems, convincingly to this conclusion.

In each case the observer, isolated in Lange's way, was given pencil and paper, and required to note down after every experiment his opinion as to the validity of that experiment. The injunction was to the following effect: "Mark all accidental sources of disturbance (stray sounds, entrance of sudden light, 'catching' of the attention by objects in the room, *etc.*), and state, as explicitly as possible, whether you have reacted as you have been directed (following all instructions as to attention to signal, resting of finger on key, position of head and body, direction of the attention during the experiment, *etc.*)." Some reagents tended very quickly to confine themselves to such notes as "quick," "slow," but this attempt to estimate the reaction in terms of time was discouraged, it being pointed out that the times were not by any means so important as the qualitative self-analysis, in which introspection (in Martius' sense) furnished a training. Between experiment and experiment there was allowed to elapse a time sufficient for the "settling-down of the mind" to a new essay, unbiased (except for *constant* influences) by those which had preceded.

[1] *Stud. from the Yale Psych. Lab., l. c.* The statement is sufficiently daring, in other connections.
[2] *Phil. Stud.*, VI. 216.
[3] *L. c.*
[4] For which *cf.* Külpe, *Grundriss*, pp. 208, 470; 44.
[5] Both in both methods of procedure, the "wissentliches" and the "unwissentliches Verfahren." *Cf. Studies, etc.*, 43.
[6] On p. 44 a position is taken by Dr. Bliss which accords more nearly with our own but which can hardly be reconciled with that of p. 36.
[7] *Phil. Stud.*, VIII. 138 ff.
[8] This JOURNAL, Jan., 1894.

We subjoin two specimen tables. In these, *b.*="bad," *g.*="good," *s.*="short," *l.*="long."

TABLE I.

Dec. 8, 1891. Reagent *Wn*.
Sensorial. Light-stimulus.
Ordinary key. Unit = 1 σ.

1. 499 *too l.*
2. 810 *quite b.*
3. 248 *g. or s.*
4. 342 *g.*
5. 341 *g.*
6. 210 *too s.*
7. 530 *too l.*
8. 331 *g.*
9. 251 *g. or s.*
10. 283 *g.*
11. 221 *s.*
12. 348 *g.*
13. 337 *g.*
14. 126 *quite b.*
15. 344 *g.*
16. 274 *too s.* [!]
17. 325 *g.*
18. 367 *l.* ?
19. 414 *l.* ?
20. 343 *g.*
21. 206 *s.* ?
22. 311 *g.*
23. 304 *g.*
24. 290 *g.*
25. 331 *g.*
26. 348 *g. or l.*
27. 278 *g. or s.*
28. 327 *g.*
29. 255 *g. or s.*
30. 292 *g.*
31. 332 *g.*
32. 273 *g.*
33. 322 *g.*
34. 420 *too l.*
35. 325 *g.*

TABLE II.

Dec. 1, 1891. Reagent *Wn*.
Muscular. Light-stimulus.
Ordinary key. Unit = 1 σ.

223 *l.*
145 *g.*
190 *g.*
213 *l.*
214 *s.* [!]
165 g.
174 *g.*
156 *g.*
289 *g.* [!]
246 *g.* [!]
259 ? ?
209 *g. or s.* [!]
142 *g.*
230 *l.*
195 *g.*
194 *g.*
199 *g.*
162 *g.*
195 *g.*
181 *g.*
139 *g. or l.* [!]
112 *g.*
146 *g.*
124 *g.*
202 *l.*
201 *g.* } [!]
203 *g.*

Table I. contains thirty-five sensorial experiments.[1] Eleven of these were rejected by the reagent.[2] If we average the remaining twenty-four (all those in the record of which the judgment *g.* occurs), we obtain the value (311,8 = reduced) 298 σ, m. v. 27,6 σ; and practically the same time-value results, if we admit (16) 274, the only case in which the observer's estimate was inaccurate. This was a series taken early in the reagent's course of practice. (His sensorial time finally averaged [reduced] 260 σ.) Introspection is already wholly adequate to the work required of it.[3]

[1] This, a practice-series, is longer than the experimental-series proper. The best length for the latter seems to be 20-25 experiments. *Cf.*, among others, Dr. Bliss, p. 17.

[2] *W.* in *Phil. Stud.*, VIII. 138.

[3] As we are not dealing here with the qualitative course of the reaction-experiment in detail, we omit from the experimental records all enumeration of the various factors which determined the reagent's judgment. Though valuable in itself, such enumeration would only serve to obscure the present issue by over-weighting the text. We may, however, repeat that the abbreviations in the text do not stand for time-judgments: "*l.*" or "*s.*" is simply an inference (of experimentee or experimenter) from the qualitative judgment or judgments entered in the reagent's protocol.

Table II. shows twenty-seven muscular times, taken from the same reagent, at a somewhat less advanced stage of practice. (Three experiments failed altogether; nothing is said of these in the table.) The time of this observer proved ultimately to be (reduced) 178,6 σ, m. v. 19,8 σ. The result of these practice experiments, according to his own protocol, would be (179,2 = reduced) 189 σ, m. v. 31,3 σ. The length of the reaction-time itself, and the magnitude of the mean variation, might be ascribed to deficiency of practice; and this explanation would hold, in the rough.[1] Looking at the figures in detail, however, we see at once that the introspective criterion is not valid. Let it be remembered that the judgments were not arrived at in terms of the time-value of the whole reaction-process, but in terms of fulfillment or non-fulfillment of experimental directions and conditions. With this standard of comparison (4) and (5) are judged as long and short respectively; (9) and (10) judged as good (these were sensorial reactions, the reagent tending, during his then stage of practice, to slip unconsciously from the muscular form to the sensorial, which was easier for him); (12) judged as short; (21) as long; (25) as long; (26) and (27) as good. Introspection is, therefore, as inadequate as it was in the former instance adequate.

It may be urged that it is unfair to make these practice-experiments the norm of experiment in general. The objection does not hold. As regards sensorial times, the introspective control certainly improves, but can improve only little, with increased practice;[2] so that Table I. is a stronger witness to its validity in this sphere than a later-taken experimental series would be. As regards muscular times, the introspective control cannot improve with practice; there is nothing present by which it should improve. The observer can only say: right or wrong, in this direction or in that. Now, when practice is complete, one obtains four general types of muscular reaction: (α) correct reactions; (β) (technically) premature reactions; (γ) (technically) false reactions, and (δ) bad reactions, due to inattention, *etc.*, or to disturbance from without. Of these (β) are as a rule regarded by the experimentee as correct; indeed, we shall admit that they *are*, in all probability, qualitatively correct reactions, if we accept Külpe's explanation of them as muscular reactions to memorial representations of the stimulus. (γ) are usually cognized by the reagent as reactions to signal, or to some interferential stimulus. Where this is not the case, they are regarded as correct; and they may be qualitatively correct—answers to the thought, "I have to react muscularly to *a* sense-impression."[3] As to (δ), external disturbances are, of course, controlled without the aid of introspection; while internal disturbances do not, as a rule, occur; so that introspection has but a very restricted field for activity. One frequently obtains an unbroken series of (α). To sum up. During muscular practice, introspection proves untrustworthy. After practice, the sphere in which the coincidence of its verdict with the objective result can be tested is too small for the drawing of a definite conclusion; the reaction tending to become automatically regular, or, when irregular, not admitting the necessary comparison.

It is needless to multiply instances. We shall quote only two other series, chosen at random from a large number.

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[1] We do not mean by this to contradict Martius. *Phil. Stud.*, VI. 190.
[2] Even so, it is crossed by the tendency to automatism in the practised reagent.
[3] *Phil. Stud.*, VI. 331.

TABLE III.

Feb. 25, 1893. Reagent *We.*[1]
Sensorial. Sound-stimulus.
Lip key. Unit = 1 σ.

1. 327 ?
2. 333 ? *l.*
3. 259 *g.*
4. 243 *g.*
5. 257 *g.*
6. 226 *g.*
7. 264 *g.*
8. 416 *l.*
9. 231 *g.*
10. 229 *g.*
11. 270 *g.*
12. 239 *g.*
13. 246 *g.*
14. 222 *g.*
15. 281 *l.*

TABLE IV.

Feb. 17, 1893. Reagent *I.*[1]
Muscular. Sound-stimulus.
Lip key. Unit = 1 σ.

154 *s.* [!]
92 *g.* ?
105 *g.*
102 *g.*
— *g.* [!]
107 *g. or s.*
127 *g.*
27 *g.* [!]
131 *g.*
118 *g. or s.*
112 *g.*

We have not enough experimental material to enable us to form an opinion with regard to the value of introspection in the case of central times.

To summarize: So far as our experience extends, we are convinced that:

1. Introspection affords an adequate control for the simple sensorial reaction;

2. While it is very much less trustworthy in its verdict upon the simple muscular reaction, the conditions of experimentation being unfavorable to its employment.[2]

(2). Are discrepant figures to be struck out in averaging? The answer to this question is already given, with the answer to the former. (α) In the case of sensorial times, it will be best to cancel all those which the reagent regards as untrustworthy; even though there are thereby sacrificed a certain number of results which accord with the established norm. (β) With muscular times it is different. All provably false reactions, and with them the obviously premature reactions (Table IV. [5] and [8]), must be put aside. For the rest, arbitrary canceling is, fortunately, rendered unnecessary by the extreme regularity of the reactions obtained from a practised observer. If obdurate results do occur, elimination by making the number of experiments very great seems to us to be the only right mode of procedure.[3]

[1] This JOURNAL, Jan. 1894.

[2] By way of a final criticism of Dr. Bliss's results under this head, we would say: (1) That the question, "Are these reactions muscular or sensorial?" (p. 37) raised after the event, can hardly be answered, except schematically, in terms of the figures and their mean variations. It should have been put earlier. And (2) that the results on pp. 33-5 appear to emanate from a too "careful" observer. It is one thing to introspect, another to inspect your introspection.

[3] *Cf.* Dr. Bliss, p. 18.

IV.

On the Quantitative Determination of an Optical Illusion.

H. W. Knox, A. B.

It is a well-known fact that, under certain conditions,[1] a dotted line appears longer than a point-distance which is objectively equal to it. We do not propose here to deal with the explanations (Hering-Kundt, Aubert, Wundt, *etc.*) proposed for the phenomenon; but rather to describe an attempted numerical determination of the special illusion, with a view to the ascertainment of regularity or irregularity in the course of its absolute or relative sensible discrimination.

Kundt[2] gives particulars of three quantitative series. He and his reagents observed the blackened ends of steel points, 30 mm. long, upon a background of white paper. Five such points were employed: A, B, C and E were kept constant; *D* was moved to and fro, until A*D* was for sensation = *D*E. The data obtained were as follows. (1) Distance of *D* from the nodal point of the observing eye, 338 mm. Two observers, *R*, *L*, and binocular experiments. Experiments massed = 240. AB = 20.2 mm.; BC = 40.2 mm.; AE = 241.9 mm.; A*D* = 117.64 mm. *Error = 6.62 mm.* (2) Distance, 328 mm. Two observers. *R* and *L* experiments. Experiments massed = 80. AB = 22.6 mm.; BC = 65.5 mm.; AE = 241.9 mm.; A*D* = 116.55 mm. *Error = 8.80 mm.* (3) Distance, 226 mm. One observer. *R*, *L*. Experiments, 40. AB = 25 mm.; BC = 10.5 mm.; AE = 100 mm.; A*D* = 48.52 mm. *Error = 2.96 mm.*[3]

A B C ←*D*→ E

Aubert used four white verticals upon a black background. The distance 1-4 = 100 mm.; 5 was moved to and fro, until 1-4 = 4-5.

1 2 3 4 ←|→ 5

The experiments, at 2000 mm. distance, gave the error 12 mm.; at 1000 mm., this amounted to 10.5 mm.; at 500 mm., to 11 mm. This latter determination makes it about one-tenth of the whole comparison-distance. Kundt's third series, the conditions of which are similar to those of Aubert's third, had made it as small as one-thirty-fourth. Aubert suggests that Kundt, influenced by knowledge of the illusion, and being his own observer, had unconsciously corrected it.[4]

Before making any remarks upon the results of Kundt and Aubert, we will set forth those obtained in our own experimental series.

New Experiments. We employed, as background, white cards, 125 x 75 mm. in size. The center of each card was the center of the whole line, composed of the dotted line (AB) and the point-distance (BC). BC was kept constant, throughout each of our four card-

[1] Not under all. If the "dotted line" is a point-distance halved by a single point, it appears smaller than the point-distance. Cf. Mellinghoff in Wundt, *Phys. Psych.*, 4th Ed. II. 142.

[2] Poggendorff's *Ann. d. Phys. u. Ch.*, 1863. Vol. 120 (196). Pp. 118 ff.

[3] These are absolute errors. Thus, in the last case, the equation (A*D*) 48.52 = (*DE*) 51.48 gives the error 2.96. It is with this absolute error that we are dealing. Kundt reckons the error from the objective center of AE; in the case cited = 1.48. He points out, rightly, that for comparative purposes it should be judged not from the objective, but from the subjective (*Augenmass*) center of a point-distance objectively = AE. Cf. Hering, *Beiträge*, 1861, I. 69.

[4] *Physiol. d. Netzhaut*, 256 ff. *Physiol. Optik.*, 630, 1. Cf. Helmholtz, *Physiol. Optik*, 1st Ed. pp. 562-3.

series. It shall, therefore, be termed C, in the experimental tables; while AB is termed V (variable). The dots in AB (marked in draughting ink with a Keuffel and Esser dotting pen) were 0.5 mm. apart, measured from center to center; and, approximately, 0.3 mm. in diameter. In *Series I.*, BC = 25 mm.; AB varied by half mm. increments from 17 to 27 mm. In *Series II.*, BC = 30 mm.; AB varied between 20 and 34 mm. In *Series III.*, BC = 35 mm.; AB varied between 24 and 38 mm. Finally, in *Series IV.*, BC = 40 mm. AB varied between 29 and 44 mm. Great care was observed that the dots might be equidistant, and of as equal magnitude and clearness as was possible.

The cards were held by the observer at the distance of clear vision (350-400 mm.), and the distances estimated by diffused daylight; the conditions of holding, of illumination, etc., being kept as uniform as possible. For the elimination of constant errors, horizontal judgments were made in equal numbers of V-C and of C-V; vertical judgments of $\frac{v}{c}$ and of $\frac{c}{v}$. The method employed was a form of the method of minimal changes. We took, *e. g.*, $V < C$, and increased it till it appeared $= C$. This value was noted down. For some few cards further, the subjective equality might persist. So soon as the limit was reached at which V appeared $> C$, its value was again noted. Similarly with $V > C$. The mean of these four lengths gave, of course, the estimation value (R) of C in terms of the dotted line.

Experiments were in no case made upon an observer for more than fifteen minutes consecutively. The manner of judgment differed with different individuals. We were able to distinguish five methods of judging. (*a*) Judgment by reference of the final dots of C and V to the center and boundary-lines of the card. Curiously enough, C appeared to increase (decrease) within limits, as V increased (decreased). [1] (*b*)Judgment by superposition of V upon C, or *vice versa*. (*c*) Judgment in terms of eye-movement. (This is the method presupposed by Wundt, in his discussion of the illusion. For the Hering-Kundt theory it is indifferent whether the eye move or not. Aubert leaves the question of the influence of eye-movement open.) (*d*) Judgment by bisection of the whole line, $V + C$. (*e*) Judgment by memorizing C, and comparing a series of V with its memory-image. The method (*a*) was employed by one subject only, and only in a few experiments. The method (*e*) must not be too sharply distinguished from (*b*). It is significant that, whatever the method of judgment, the illusion remains. How small a part is played by individual differences in this regard will be shown by our tables. As a strict control of the method of judgment was impossible, it seemed best to allow each observer to follow the line of least resistance. The result was that the principal types,—(*b*), (*c*) and (*d*),—were not infrequently employed in common for the formation of one and the same judgment.

We found that there was manifested in some observers a tendency to regard C as the distance from inside edge to inside edge of the limiting dots, while V was measured from the outside edge of one to the outside edge of the other. In computing our results, however, we measured both lines from dot-center to dot-center. Each dot is approximately 0.3 mm. wide. C was, therefore, seen too short by 0.3 mm.; V too long by the same amount. Now the rela-

[1] This suggests an experimental method for the quantitative investigation of apperceptive completion (Wundt, Münsterberg, Külpe, Grützner, *etc.*). Experiments on this subject are at present being carried out in the Cornell laboratory.—E. B. T.]

tive difference-limen for moderate distances is put by Külpe and others at one-fiftieth.[1] A distance of 25 mm. would, therefore, be just noticeably different from one of 24.5; 30 from 29.4; 35 from 34.3; 40 from 39.2. Our values were 24.6, 29.6, 34.6, 39.6. The difference of each of these from its computation-value is, therefore, subliminal, and may be neglected. *C* may stand at 25, 30, 35 and 40 mm. What of *V*? Shall we read, for a *V* of 22, 22.3; for a *V* of 28, 28.3? Plainly, 22 is just noticeably different only from 22.44; 28 from 28.56; *etc.* Again, the difference of the actual from the computation-values is subliminal; the latter may stand. To say that *C* of 25 = *V* of 22, and to say that *C* of 24.6 = *V* of 22.3 are, for psychophysical purposes, the same. It is not as if there were summation of errors, and *C* of 24.3 were compared with *V* of 22.6. In that case, 24.3 would be noticeably different from 25; though 22.6 would not differ from 22. We have mentioned this tendency, although it does not affect our results, because, as can readily be seen, it might under certain conditions of judgment exercise a considerable influence, and demand recognition in the final computation.

All our experiments were made binocularly, and without restriction as to eye-movement. Fairly complete series were obtained from six subjects: Miss Healy (*H.*), Miss Washburn (*W.*), Messrs. Knox (*K.*), Pillsbury (*P.*), Titchener (*T.*) and Watanabe (*Wa.*). The results are subjoined. Under *R* are given the estimation values of the four constants. The *n* column shows the number of records taken, each record including the determination of r_o', r_o'', r_s' and r_s'', from which the average was calculated. Under the rubric *m. v.* is given the total mean variation of each separate *R*, calculated from the whole number of experiments.[2] The mean variation of R_1, R_2, *etc.*, from each *R*, it seems unnecessary to give in a separate column; for the first three constants it lies between one and five tenths mm.; for the last it averages five-tenths,—*W.* having the largest *m. v.*, 1.2 mm., and *Wa.* the smallest, 0.1 mm.

TABLE I.

Reagent *H.* Slight astigmatism[3]. Method *(c)* predominantly follows. Special practice only. Unit = 1 mm.

SERIES.	*R.*											
	C=25	*n.*	*m. v.*	*C*=30	*n.*	*m. v.*	*C*=35	*n.*	*m. v.*	*C*=40	*n.*	*m. v.*
C—*V*	22.25	3	1.5	26.95	3	2.25	31.41	3	2.58	36.91	3	1.91
V—*C*	22.00	3	2.0	26.25	3	2.25	30.75	3	2.25	36.91	3	2.08
$\frac{V}{C}$	22.83	3	1.6	27.31	3	2.75	30.83	3	2.00	37.00	3	2.08
$\frac{C}{V}$	22.83	3	1.6	27.33	3	2.00	32.00	3	2.00	37.08	3	2.25
Hor.△[4]	—2.88			—3.40			—3.92			—3.09		
Vert.△	—2.17			—2.68			—3.59			—2.96		

[1] *Grundriss*, pp. 371-2. Wundt, *Vorlesungen*, 2d Ed. p. 162.
[2] This is, of course, not a true m. v. But it is sufficiently accurate for our purpose. Cf. Gruber, Ueber die specifische Helligkeit der Farben, *Phil. Stud.*, IX. p. 435.
[3] Corrected by glasses.
[4] $\Delta = R - r$.

TABLE II.

Reagent *W*. Very slight astigmatism[1]. Methods *(a)* and *(c)*. General and special practice. Unit = 1 mm.

SERIES.	R. C=25	n.	m. v.	C=30	n.	m. v.	C=35	n.	m. v.	C=40	n.	m. v.
C—V	23.00	3	1.08	27.66	3	1.45	32.50	3	1.25	38.16	3	1.33
V—C	23.33	3	0.91	27.08	3	0.95	31.25	3	1.33	36.83	3	1.16
$\frac{V}{C}$	23.16	3	0.91	27.75	3	1.00	32.13	3	1.08	36.50	3	1.00
$\frac{C}{V}$	22.75	3	0.93	27.75	3	1.00	32.50	3	1.25	37.50	3	1.08
Hor.△	—1.84			—2.63			—3.13			—2.15		
Vert.△	—2.05			—2.25			—2.69			—3.00		

TABLE III.

Reagent *K*. Vision normal. Method *(e)*. General and special practice. Unit = 1 mm.

SERIES.	R. C=25	n.	m. v.	C=30	n.	m. v.	C=35	n.	m. v.	C=40	n.	m. v.
C—V	22.50	3	0.62	27.33	3	0.91	32.50	3	0.75	38.16	3	1.33
V—C	23.08	3	0.76	27.91	3	0.75	33.08	3	0.83	39.00	3	0.66
$\frac{V}{C}$	23.25	3	1.00	28.25	3	0.75	33.58	3	0.66	37.75	3	0.66
$\frac{C}{V}$	23.33	3	0.79	27.41	3	1.00	33.00	3	0.87	38.08	3	0.91
Hor.△	—2.21			—2.38			—2.21			—1.42		
Vert.△	—1.71			—2.17			—1.71			—2.09		

[1] Not sufficient to call for correction by glasses.

TABLE IV.

Reagent *P*. Vision normal. Method predominantly *(b)*. General and special practice. Unit = 1 mm.

SERIES.	*R.*											
	C=25	*n.*	*m. v.*	*C*=30	*n.*	*m. v.*	*C*=35	*n.*	*m. v.*	*C*=40	*n.*	*m. v.*
C—V	23.28	16	0.63	28.25	16	0.64	32.98	16	0.75	37.75	16	0.72
V—C	23.82	16	0.69	29.25	16	0.63	34.04	16	0.68	39.00	16	0.59
$\frac{V}{C}$	23.10	10	0.62	27.60	10	0.56	32.34	10	0.48	36.87	10	0.68
$\frac{C}{V}$	23.38	10	0.56	27.72	10	0.60	32.67	10	0.50	36.82	10	0.77
Hor.△	—1.45			—1.25			—1.49			—1.83		
Vert.△	—1.76			—2.34			—2.50			—3.16		

TABLE V.

Reagent *T*. Vision, *L*., normal; *R*., myopic[1]. Method predominantly *(b)*. General and special practice. Unit = 1 mm.

SERIES.	*R.*											
	C=25	*n.*	*m. v.*	*C*=30	*n.*	*m. v.*	*C*=35	*n.*	*m. v.*	*C*=40	*n.*	*m. v.*
C—V	20.96	8	0.87	24.03	8	0.96	28.62	8	0.93	33.90	8	0.87
V—C	20.62	8	0.78	24.40	8	0.82	29.18	8	0.87	34.56	8	0.81
$\frac{V}{C}$	20.20	5	0.65	23.07	5	0.70	28.05	5	0.85	33.25	5	0.85
$\frac{C}{V}$	20.55	5	1.00	24.40	5	0.70	28.15	5	9.95	34.15	5	0.85
Hor.△	—4.21			—5.79			—6.10			—5.77		
Vert.△	—4.63			—6.27			—6.90			—6.30		

[1]See below, *Remarks* (5).

TABLE VI.

Reagent *Wa*. Vision slightly myopic. Method mixed. General and special practice. Unit = 1 mm.

SERIES.	R.											
	C=25	*n.*	*m. v.*	*C*=30	*n.*	*m. v.*	*C*=35	*n.*	*m. v.*	*C*=40	*n.*	*m. v.*
C—V	23.41	3	0.58	28.75	3	0.66	32.91	3	0.58	39.55	3	0.58
V—C	23.75	3	0.41	29.25	3	1.58	33.08	3	0.58	37.91	3	1.08
$\frac{V}{C}$	22.41	3	0.66	26.00	3	0.75	33.33	3	0.58	35.91	3	0.25
$\frac{C}{V}$	24.33	3	0.83	30.16	3	0.75	34.58	3	0.50	39.25	3	0.50
Hor.△	—1.42			—1.00			—2.01			—1.27		
Vert.△	—1.63			—1.92			—1.05			—2.37		

Remarks. We notice (1) that the illusion holds for every observer, for both vertical and horizontal judgments, and whatever the method of judgment.

(2) Since our judgment of vertical distances is in general less accurate than our judgment of horizontal (Chodin, Volkmann), we should expect to find a higher value of △ in the former case than in the latter. The twenty-four comparisons of our tables give fourteen confirmations, ten refutations of this expectation. Two of the contrary cases, however, may be regarded as neutral (2.83, 2.17; 2.63, 2.25): so that we have fourteen *r*, eight *w*, two =. Of these eight, four belong to Table I.; two to Table III.; one each to Tables II. and VI. The results of Table I. were taken (*a*) from a previously unpractised subject, who (*b*) began with horizontal judgments. These latter, therefore, may well be regarded as belonging to a stage of less complete practice than the vertical judgments. We have, then, in conclusion, fourteen *r*, four *w*, two =. When we consider the fewness of our experiments, we cannot but think that this is as strong a confirmation of our expectation as could have been hoped for.

(3) Binocular bisection of horizontal distances is not subject to any constant error; binocular bisection of verticals is subject to the constant error of over-estimation of the upper part of the field of vision.[1] We should, therefore, expect to find the *m. v.* of our vertical △'s greater than that of our horizontal. The results, if Table I. is omitted, are: *r* 6, = 7 (.99, .92; .79, .76; .66, .59; .63, .58; .82, .82; .90, .85; .58, .54), *w* 7. [Table I. gives *r* 2, = 1 (1.7, 1.6), *w* 1.] This is curious. We are unable to offer any explanation of the result.

(4) Is there any constancy of the relative or absolute sensible discrimination? Of the latter, obviously not. The values of $\frac{\triangle}{r}$ are:[2]

[1] Wundt, *Grundzüge*, 4th Ed., II. 139, 140.
[2] *Op. cit.*, I. 343.

I. *Hor.*: $\frac{1}{8}$-$\frac{1}{9}$, just over $\frac{1}{9}$, just over $\frac{1}{9}$, just over $\frac{1}{13}$. *Vert.*: $\frac{1}{11}$-$\frac{1}{12}$, $\frac{1}{11}$-$\frac{1}{12}$, $\frac{1}{9}$-$\frac{1}{10}$, $\frac{1}{13}$-$\frac{1}{14}$.
II. *Hor.*: $\frac{1}{11}$-$\frac{1}{14}$, $\frac{1}{11}$-$\frac{1}{12}$, $\frac{1}{11}$-$\frac{1}{12}$, just over $\frac{1}{13}$. *Ver.*: $\frac{1}{13}$, $\frac{1}{12}$-$\frac{1}{14}$, $\frac{1}{12}$, $\frac{1}{13}$.
III. *Hor.*: $\frac{1}{11}$-$\frac{1}{12}$, $\frac{1}{12}$-$\frac{1}{13}$, $\frac{1}{15}$-$\frac{1}{16}$, $\frac{1}{28}$. *Vert.*: $\frac{1}{14}$-$\frac{1}{15}$, just over $\frac{1}{14}$, just over $\frac{1}{21}$, $\frac{1}{15}$.
IV. *Hor.*: $\frac{1}{17}$, $\frac{1}{24}$, $\frac{1}{23}$-$\frac{1}{24}$, $\frac{1}{23}$. *Vert.*: $\frac{1}{14}$, $\frac{1}{12}$-$\frac{1}{13}$, $\frac{1}{14}$, $\frac{1}{12}$-$\frac{1}{13}$.
V. *Hor.*: $\frac{1}{8}$, $\frac{1}{8}$-$\frac{1}{9}$, $\frac{1}{8}$-$\frac{1}{9}$, just over $\frac{1}{7}$. *Vert.*: $\frac{1}{8}$-$\frac{1}{9}$, $\frac{1}{8}$-$\frac{1}{9}$, $\frac{1}{8}$-$\frac{1}{9}$, $\frac{1}{8}$-$\frac{1}{7}$.
VI. *Hor.*: $\frac{1}{17}$-$\frac{1}{18}$, $\frac{1}{20}$, $\frac{1}{14}$-$\frac{1}{15}$, $\frac{1}{21}$-$\frac{1}{22}$. *Vert.*: $\frac{1}{15}$-$\frac{1}{16}$, $\frac{1}{15}$-$\frac{1}{16}$, $\frac{1}{22}$, $\frac{1}{17}$.

This apparent chaos reduces itself to some order on close inspection. To notice are the following facts. (*a*) The values of Table I. are all too large, for the reason already given. Moreover, here the vertical discrimination is (contrary to rule) finer than the horizontal. An explanation of this fact has been also suggested above. (*b*) The irregularities of Table III. may be referred to the fact that the reagent *K.* was the experimenter in all the experimental series of the other tables, and had himself prepared the cards employed in experimentation. (*c*) The smallness of certain fractions in Table IV. may be due to the very large number of experiments made with the reagent *P.* This number was probably *too* large. One of the difficulties in the way of the quantitative determination of an optical illusion is just this, — that the observer must be practised enough to be a good discriminator, but not so practised that he minimizes the illusion by correction. (*d*) Table V. we shall deal with separately, later. (*e*) The $C = 40$ mm. gives throughout a smaller fraction than we should expect. The reason for this does not, however, lie in any of the conditions of the illusion under investigation, but in the nature of our apparatus. Our cards were 125 mm. long. The 40 mm. series of lines varied between 69 and 84 mm. The average length of the line, the dotted part of which was judged equal to 40 mm., was 77 mm. Plainly, the terminal dots of this are dangerously near the card-edges: the distance on either side amounts only to 24 mm. When the cards were being prepared, the question was raised as to whether the 40 mm. series could safely be introduced, or whether it would not be better to use a larger sized card. We decided to test the matter; and our results show decisively the determining influence of the too-near limiting line. (*f*) Table VI. is puzzling. Want of practice will not explain the heterogeneousness of the $\frac{\triangle}{r}$ values; for Table I. shows a clear uniformity, on the basis of very much less practice. We are inclined to suspect a more or less sporadic tendency on the part of the reagent to correct the illusion. Such a tendency is hard to guard against; and conscious avoidance of it may easily lead to exaggeration of the illusion.

Premising that the values of Table V. are explicable, and taking into account all the facts above enumerated, we would conclude from the experimental results,—though quite tentatively, and subject to correction by further investigation,— that *within the limits $C = 25$-40 mm. there is a constancy of $\frac{\triangle}{r}$, at about $\frac{1}{13}$ for horizontal estimation, and at a somewhat higher value for vertical.* For the latter determination, see, especially, Tables IV. and V.

(*b*) How are the results of Table V. to be explained? We notice (*a*) that it is a typically good table, as regards the illustration of the uniformities which we have discussed. $\frac{\triangle}{r}$ is constant; the vertical $\triangle$'s are greater than the horizontal; discrimination is affected by the "boundary-error" in the 40 mm. judgments; the

m. v. shows 1 *r*, 1 *w*, and 2 = cases. (*b*) But the numerical values of $\triangle$ and $\frac{\triangle}{r}$ are far too high.

The reagent *T.* proposed and planned the investigation. He had more familiarity than any of the other five subjects with the phenomena of optical illusion. He, therefore, approached the experimentation with a definite expectation of what would, in general, result from it; and he endeavored to give the illusion full play, being aware of the tendency towards correction which is apt to arise in the course of a sustained consideration of similar phenomena. It may be that this expectation and endeavor combined to bring about an exaggeration of the illusion; just as the knowledge and practice of the reagent *P.* combined to decrease it.

But *T.*'s vision is also not normal; *L.* being emmetropic, *R.* myopic.[1] All the judgments of Table V. were made with the naked eye; *i. e.*, monocularly, by *L.* Could this fact have influenced the estimation? In the cases of horizontal distances, Kundt was able in Series I. to mass his *L.*, *R.* and binocular results, (i) for a far-sighted subject, (ii) for his own unequal (*normal-kurzsichtig*) vision.[2] In Series II. he massed two sets of judgments, one made principally with *L.* the other with R.[3] Series III. gave a subliminal difference for his own unequal *R.* and *L.*[4] (*L.* = 48.26 mm.; *R.* = 48.78. AE = 100.) In determining the subjective middle of AE = 100, as point-distance, he found that the *L.*-judgment gave 50.33 for the left half, the *R.*-judgment 49.845 for the same half.[5] He observes, after calculating the *R.* and *L.* errors of Series III. on this basis, that "sich die Verschiedenheit meiner Augen wieder sehr bemerklich macht;"[6] but it is plain that the "very noticeable difference" is quite inadequate to explain the figures of our Table V. We must, therefore, lay the main stress upon the explanation which we first suggested.

Supplementary Remarks. (1) We have already noticed Mellinghoff's statement, that a point-distance halved by a dot appears smaller than a simple point-distance, which is objectively equal to it. It would be interesting to investigate systematically the quantitative relation of the illusion under discussion to the number of dots. We prepared a series of cards, upon which were two point-distances, *AB* and *BC*, of 30 mm. length. *BC* was varied in ten ways: left open, halved by one dot, divided up at equal intervals by 2, 3 . . . 9 dots. Some ten judgments gave the following constant result:—

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[1] Formula for reading glasses: *L.*, convex, 0.25 + *R.*, concave, 1.25 dioptr. For long-distance glasses: *L*, concave, 0.00 + *R.*, 2.75 dioptr.
[2] *L. c.*, p. 132.
[3] P. 133.
[4] P. 134.
[5] P. 135.
[6] P. 136, note.

Dots in *BC*	REAGENT: H.	K.	P.	T.	Wa.
0	=	=	=	=	=
1	=	−	−	−	−
2	=	+	+	+	=
3	+	↓	↓	↓	Small +
4	↓	↓	Illusion constantly increases	Maximum +	"
5	↓	↓		↑	"
6	Maximum +	Maximum +		↑	"
7	↑	↑		↑	"
8	↑	↑		↑	"
9	+	+		+	"

Obviously, here is a problem, the investigation of which promises to bring to light new uniformities.

(2) *Practice.* The constancy and extent of the influence of practice on these experiments cannot be better shown than by the presentation of a series of observations from the results of the observer *P.* The series is $V\text{-}C$; $C = 25$.

Oct.	12, 1893.	*R.* = 22.87	m. v. = 0.62
"	18,	23.00	0.50
"	20,	23.12	1.12
"	23,	23.25	0.62
"	25,	24.00	0.75
"	26,	23.87	0.87
"	28,	24.12	0.75
Nov.	2,	23.37	1.37
"	3,	24.50	0.75
"	5,	25.12	1.12
"	9,	24.87	0.43
"	10,	24.37	0.43
"	11,	24.87	0.62
Dec.	6,	23.75	0.50
"	8,	24.75	0.25
"	10,	24.00	0.50
		etc.	

Practice has already been discussed above: *Remarks* (4).

(3) Can we compare our $\frac{\triangle}{r} = \frac{1}{13}$ with the values obtained by Kundt and Aubert? Aubert's $\frac{1}{10}$ is tempting; but he was dealing with vertical lines, not with points. And his filled space contained only two verticals. We have seen, under (1), that the character of the filling is essential in the illusion. Kundt employed points, it is true, but his distribution of them was curiously arbitrary. And his experiments are, on other grounds, hardly comparable with our own; so that the divergence of the two sets of results need not trouble us.

[Further experiments upon this special illusion are in progress in the laboratory.—E. B. T.]

V.

Some Apparatus for Cutaneous Stimulation.

A. By M. F. Washburn, A. M.

Fig. 1.

1. The first *Æsthesiometer* appears to be that described and figured by Czermak (*Physiol. Studien*, III. *Abth.*, 1855). Later instruments have followed the model of this, with the exception that both points are usually fixed at the same horizontal level, whereas in Czermak's *Stangenzirkel* "one limb (the terminal) is shorter than the other, and movable in the vertical direction." This latter arrangement allows of the application of two simultaneous pressures to a curved surface. But the general objection to the æsthesiometer—that there is no guarantee of constancy of stimulation-intensity from experiment to experiment—holds of all the slide-rule forms of the apparatus.

It appeared to us that the objection could be best met by a modification of the instrument, which would allow the two points to fall by their own weight, and with approximately constant friction, through a fixed support. And mechanical considerations narrowed this plan down to that of allowing a single rod, carrying the two points, to fall through such a support. The manner of constructing and affixing to the rod the cross-bar in which the points are inserted was suggested to us by the rough instrument employed by Professor Jastrow in his anthropometrical laboratory at the World's Fair.[1] The new æsthesiometer took final shape as follows.

Figure 1.

A is a heavy brass bulb, for holding in the hand. Through it passes a light steel rod, *B*, threaded at either extremity. The upper end carries any chosen one of a series of circular weights, *C*. The lower end is screwed into any chosen one of a series of blocks of hard rubber, *D*, which carry two rubber points, accurately distanced, and accurately turned to a final diameter of one-third mm. *B* is grooved over a certain portion of its length; and in the groove runs the point of a screw, *E*, the head of which is level with the surface of the brass bulb. The Cornell laboratory possesses a series of *D*, from a single point to a point-distance of 28 mm., the increments being 1 mm.; two *AB*; and weight-combinations from 15 to 90 gr. The dimensions of the whole apparatus can be chosen at pleasure.

The great advantages of this form of the æsthesiometer are: (1) The practical constancy of pressure, as between point and point, and as between experiment and experiment; if *B* is kept clean and well-oiled, its friction with *A* is minimal. (2) The absolute

[1] Professor Jastrow's perfected instrument is figured and described in the catalogue of the Garden City Model Works, 124 Clark street, Chicago, Ill., p. 1. The form with adjustable points has the disadvantage that the points themselves are formed each by a vertical and an inclined line, instead of by two lines equally inclined to the horizontal plane.

accuracy of the point-distances; there is no room for error of adjustment. Its disadvantages are: (1) Its restriction to the method of right and wrong cases; it can hardly be employed for minimal changes, unless two experimenters take part in the work done with it, and there are two *AB*, and a very long series of *D*— or unless *D* be remodeled, on the old slide-rule plan. (2) Its expense. (3) The fact that, at any other than a very moderate inclination of *AB* from the vertical direction, constancy of pressure is not obtained.

For demonstration experiments, *D* may be replaced, either by a rubber bar carrying roughly-pointed pyramids of hard rubber, as in Professor Jastrow's original apparatus; or (perhaps better) by a bar, *e. g.*, of wood, carrying a piece of sheet-rubber (one-hundredth inch) accurately cut to fine points at a definite distance from one another.

The instruments were made by the Pratt & Whitney Co., Hartford, Conn.

2. For *punctual* and *areal* cutaneous exploration, various instruments have been proposed, *e. g.*, by von Vintschgau (*Pflüger's Archiv*, X. 4. XVI. 318), Goldscheider (*du Bois-Reymond's Archiv*, 1887, 428), Dessoir (same *Archiv*, 1892, 308). After several failures, we have succeeded in constructing a fairly good, though by no means ideal, apparatus for the purpose. It is as follows.

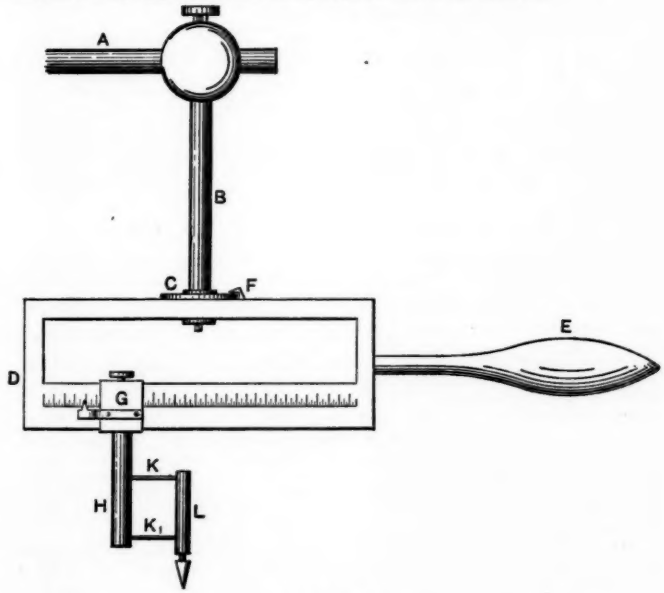

FIGURE 2.

A is a steel bar, movable in the vertical direction along the standard to which it is attached. *B* is a similar bar, movable around *A*. *C* is a fixed brass disc, divided up into degrees. *D* is a frame

of hard rubber, rotatable by the handle E around the extremity of B, and carrying a pointer F, which indicates the extent and direction of its movement, in terms of the scale marked upon C. The lower edge of D is provided with a mm. scale. Along this scale runs, in either direction, the brass carriage G whose pointer can be set at any number upon the scale. Attached to G is the brass rod H; and this latter carries on two stiff steel springs, K and K_1, the rubber point L. By means of L the skin can be explored, in circles of continually increasing diameter. The springs K and K_1 prevent any catching of L by inequalities of the cutaneous surface. "Pressure spots," *etc.*, can be accurately mapped by reference to the scale upon the disc C; and, if any one point of the stimulated area is known, a series of experiments can be repeated with complete accuracy of localization from day to day.

The introduction of K and K_1 is essential. An arm, revolving round the lower extremity of B, and carrying L directly, is unworkable; even if a spline-and-feather be inserted in B, the slight roughness of the superficial skin causes in this case a dragging of L, and prevents exact localization.

The point of L may be made detachable from its stem, to allow of the application of various forms (areal and punctual) of stimulus (for temperature, pressure, pain). A spring-point enables the instrument to be used as a sensibilometer.

This instrument was made partly by C. Krille, Leipzig, partly in the Yale and partly in the Cornell workshop. Its cost should not exceed $10.

For *linear* cutaneous stimulation, triangles may be cut from sheet-rubber (one-hundredth inch) and their apices inserted in light wooden handles. It is very easy to construct these simple instruments in sufficient numbers to admit of the employment of the method of minimal changes. Constancy of pressure must be learned by practice, as in the case of the ordinary æsthesiometer.

B. By E. W. Scripture (Yale University) and E. B. Titchener.

3. The *Kinesimeter* of Hall and Donaldson (*Mind*, X. 403) is a very imperfect instrument. The following criticisms suggest themselves:

(*a*) To start a machine by interlocking gears, means a variable error of $e = \pm \frac{t}{2g}$; where g is the number of teeth in the started gear, and t the rate of revolution of the motor gear; *e. g.*, if the motor gear make one revolution in two seconds, and move a gear with twenty-four teeth, the speed of the latter at the moment of starting dt will vary between $\pm$ one-twenty-fourth second. With a slow-moving car, it is sometimes possible actually to see the car move backward for an instant. As the speed increases, the error is diminished; but in the slow-moving kinesimeter it is very large. This objection is a fundamental mathematical one. In practice, no machines are started by interlocking gears where other methods can be employed.

(*b*) This variable error is thrown into the shade by the large constant error introduced, when the gears are not interlocked by a movement in the line connecting their two centers. Any movement around an axis simply pushes the second gear forward or backward. On a reproduction of the original instrument, this error amounted to about one-eighth inch in the movement of the car; the radius for the interlocking movement being about one and one-half inches. With the slow speeds used, the error would be rendered

negligible only by the employment of a radius of three feet or more.

(*c*) The stretching of the belt for the car was affected by placing the end-pulley on a spring. Every change of friction in the car would produce a change in the spring, a relaxation of the belt and a jerk in the movement of the car.

(*d*) To start a machine suddenly by connection with a kymograph throws more work on the latter, and lowers its speed, until the governor has time to react. During this time, therefore, the motion of the car is too slow.

(*e*) When the moving point meets with an elevation upon the cutaneous surface, it rotates upward; when it meets with a depression, downward. The upward movement differs but little from the horizontal; the downward movement is greatly accelerated. Between the fastest and slowest rates, for a nominally constant rate given by the car, there is a wide interval; the *m. v.* is very large.

[(*f*) In the presentation of the results of the investigation, nothing is said as to these errors and constants of the apparatus; no measurements of its accuracy are given; there is not even a statement of the *m. v.* of the final result.]

In constructing a kinesimeter for the Cornell laboratory, we have introduced the following improvements:

(*a*) This error has been eliminated by the employment of friction-gears. The difficulty caused by the increased pressure of the wheels against their axis is removed by allowing two friction-wheels to clasp a third.

(*b*) This error does not enter into friction gears.

(*c*) This error is eliminated by tightening the belt with a screw-fastening for the pulley, as in machines in general.

(*d*) This error can only be avoided by the employment of a heavy fly-wheel on a vertical axis, run by some form of small motor (water, electricity, kymograph, *etc.*).

(*e*) This error is partially eliminated by the direct up-and-down movement of an extra point added.

The new instrument in the Cornell laboratory was built in the Yale workshop. It can be supplied at the cost of $90—$100. Its salient features are as follows. (1) *Table.* Of brass casting, hard metal. Top planed perfectly true; V's planed at the same time as table. Legs fastened by four screws. One leg has adjusting screw, to compensate any unevenness of support. Nickel plated, with coating of copper first. (2) *Car.* Brass, nickel plated. Four wheels, turned on a perfectly true arbor. Bearings are bushings of hardened tool-steel; holes ground and lapped, so as to ensure perfect trueness in running. Side-looseness of car easily taken out, and wheels easily taken off their bearings. Shaft of hardened tool-steel, ground and lapped; perfectly true and round. Horizontal slide holding vertical rod readily adjustable by special mechanism. Vertical rod itself (carrying rubber stimulus-point, wheel, tube, or whatever is preferred) held in position by brass nut clamping conical head of support on horizontal slide. (3) *Rotating Power in Head.* Comprises three gears and three friction rolls. Held in two brass plates, nickel plated; bearings are hardened bushings, set firmly in place. Movement of car regulated by lever; two directions and rest. Pressure of driving rolls against principal roll maintained by two springs, the tension of which can be adjusted by two brass nuts on the end of a bar connected with lever. Since the rolls are held together by spring-pressure, and not by a positive stiff joint, any unevenness (dirt on surface, *etc.*) will not affect the movement. Rolls perfectly true, turned on centers. By moving top plate, the whole driving section can be

taken apart. Rubber pulley held on its bearing by small screw on top, with conically shaped head. Shaft counter-sunk to fit; tightening screw expands slotted end of shaft, and the pulley is so held firmly in position. Three notches in support holding reversing lever. Center notch keeps car at rest. Balanced pointer mounted on two hardened centers. Case in which gear is held entirely independent of outside case holding large friction roll. Pressure against driving roll will not interfere with shaft of rubber pulley, which is mounted on bushings of hardened steel; the friction obtained by pressing the rolls together has nothing to do with running of this shaft, the two pressures being independent. Endless cord propelling car is joined by a knot, fitting into a rest and slot affixed to the car. Adjustment for tightening cord is a pulley at the other end of the table, with thumb-screw beneath.

This kinesimeter has been thoroughly tested on the smoked paper of the kymograph. It was found to be absolutely accurate, to the degree of accuracy obtainable from the kymograph.

ACCURATE WORK IN PSYCHOLOGY.[1]

By E. W. Scripture, Ph. D., Yale University.

There are periods in the life of a science when it becomes necessary to take a decided stand against the tendencies prevailing at the place and time, when the battle of the moment is not against avowed enemies, but against the very ones that apparently support the cause. The danger that threatens us comes directly from the psychological laboratories. We make experiments without an idea of the first laws of experiment; we make measurements regardless of the fact that a science of measurement exists; we use apparatus without knowing the principles of its construction. Instead of keeping all the conditions, except one, in a series of experiments nearly constant, we vary a lot of them at the same time, often not knowing which ones are varied, and we assume to make deductions from the results of such work. Instead of carefully eliminating or evaluating the sources of variation in our measurements, we prefer to manipulate the results as we think best. Instead of studying our apparatus with the utmost care to find the constants of errors, we prefer to consider that our results are only relative anyway and constant errors do not count.

The struggle for ever increasing accuracy is the vital principle of all the sciences. No astronomer, physicist or biologist would for an instant hesitate to declare that his work aims at the employment of ever more careful methods.

How different is the case in psychology! We frequently hear it stated that psychological experiments and measurements can never be exact or trustworthy; at best they can only give an inkling of the facts of the case. The deduction made is that any particular care or apparatus is unnecessary, all the work can be done by any amateur who has fifty cents to spend for some colored paper. The consequence is that, although we have psychological laboratories on every hand, with new ones added every year, almost all the scientific work of any value comes from two or three laboratories alone. The instruction in psychology is about on the level of "easy lessons for beginners in chemistry," or "physics in the parlor." Such instruction has its value in its place, but the really scientific pursuit of physics and psychology is quite a different matter. I will say nothing about the necessity of long training in the laboratory,[2] of the need of proper equipment in the way of rooms, apparatus, etc.; but I wish to call attention to a fundamental misconception by many American psychologists. This I will do by using a single example for illustration; the reader can

[1] Address delivered at the second annual meeting of the American Psychological Association, New York, 1893.

[2] Science, 1892, XIX. 127.

easily extend the application. It is now nearly a century since Maskeleyne discovered that our mental activities required time for their occurrence. Hundreds of investigations have been made on its varieties and complications, and, yet, — with the greatest care that we can exercise to-day, our measurements of the simplest form of reaction-time give a mean variation of 10σ to 12σ. Putting the average of such a reaction-time at 180σ, this means a mean error of, say, 6%, — a degree of inaccuracy that would not be tolerated in physics. Moreover, we know that these variations are due largely to such factors as distraction of attention, fatigue, general conditions of time and place, errors of apparatus, etc., none of which we attempt to evaluate or to more than roughly eliminate. As soon as we leave the simplest forms of reaction-time the mean variation becomes much larger; when we get to the complex forms of reaction, we sometimes do not know what we are measuring.

What would a true scientist do in such a case? In the first place, years of labor would be spent in getting the sources of variation under control. Scientist after scientist would seek means of controlling the amount of distraction to ever greater fineness and of measuring the effects of the various degrees of distraction on the reaction-time. Still others would step by step evaluate or eliminate the effects of different degrees of fatigue; others would manage to make the conditions of time and place practically constant. The story of such a gradual elimination of sources of error in astronomical recording, reaching over 3,000 years, is told by Jevons.[1] The subject of calorimetry will indicate how physicists go to work; the endless investigations on nerve irritability and conduction furnish a parallel case for physiology. Let us turn to psychology. Year after year, measurements on the time relations of mental phenomena go on at Leipzig with ever increasing accuracy and with the continual discovery of unsuspected facts. Investigations on new subjects that can be treated at first only qualitatively, are also taken up; *e. g.*, experimental æsthetics, association of ideas, etc., but the fact is always kept in mind that each subject taken up should be pushed forward from time to time, ever a little further into the domain of accuracy. And in America? — From one of the leading laboratories we have the bold declaration that accurate work is not even desirable. More than once have the results of measurements been published without a statement of mean variations or probable errors, or of any data that would enable the reader to judge whether the methods were accurate enough to justify the conclusions drawn or not. Wundt considers such amateurism and carelessness to be the most dangerous enemies of modern psychology. It is this tendency to superficiality and dabbling, so common among American students, which must be eradicated at the beginning of their laboratory instruction. In the work of the Yale laboratory the attainment of accuracy and trustworthiness is made the indispensable condition in all research-work.

The illustration given above will serve to indicate the methods of search for errors and the labor spent on their elimination, as they are followed at the Yale laboratory:

1. Errors of the apparatus. The Hipp chronoscope, with which these experiments are usually made, has been rejected as not accurate enough. Aside from the errors due to remanent magnetism, springs, etc., the very construction of the catch-pin (*b.* in Wundt's *Physiologische Psychologie*, 4th ed. II. fig. 217) involves a mean

[1] "Principles of Science," p. 271.

variation of 1σ. We have, therefore, rejected all chronoscopes and developed the graphic method to such a degree that we can make and count records of any desired accuracy with less work than with the Hipp chronoscope.[1] All errors from time-markers have been eliminated by using the spark method. In fact, the degree of accuracy attainable is limited only by the accuracy of the tuning-fork. By an alarm-thermostat placed beside the fork, even the error due to changes of temperature is kept below $\frac{1}{10,000}$ of a second. Thus our apparatus is absolutely accurate for all records in thousandths of a second. I hardly need to say that we use only break-circuit keys for stimulating and reacting. To avoid errors due to muscular tension, we use our specially constructed reaction-key, that has no spring and that can be held in any position.[2]

2. Errors of surroundings. Of course, the experimenter, the recording apparatus and the stimulating apparatus are in a part of the building distant from the person experimented upon. He sits in the reacting room perfectly alone, knowing nothing of what is going on. The warning click of a sounder tells him to concentrate his attention; a click occurs in the telephone, or a Geissler tube flashes out, or an electric shock pricks the skin; he reacts in response and all is again quiet. All light and moving objects are, of course, excluded. Dr. Bliss's experiments have shown that a steady light of moderate intensity causes no distraction; we may consequently at the present stage of accuracy have the room lighted up by an incandescent lamp, if the observer is made more comfortable thereby. Disturbing sounds are probably the worst sources of error; their exclusion has been a difficult problem, but we have solved it by our isolated room. The distractions due to insufficient ventilation, changing temperature, changing barometric pressure and changing humidity have not yet been eliminated, but the arrangements thereto have been made and will be completed before long.

3. Errors due to poor powers of introspection. Trustworthy observation on the fundamental laws of mental life can be made only by trained observers. Let me show you a picture for a moment and then remove it. How much of that picture can you remember? If I had shown it to an average individual, the amount remembered would have been very small; yet you all know the story of Robert Houdin and his son, who by painstaking training were finally able to tell every article in a shop-window after casting a momentary glance at it,—the son, indeed, was able to glance for an instant over the shelves of a library and then tell from memory the title, cover and position of each book.

You had the picture before you for a few seconds; how much more difficult must it be to carefully observe and remember the rapidly changing phenomena of consciousness. The conclusion seems self-evident. You would not put a car-driver to seeking double-stars with the Lick telescope, and I think you will agree with Prof. Titchener, who claims that thorough psychological work can be done only by those trained in introspection. Indeed, it would seem unnecessary to mention the matter, if it were not for the wide-spread impression that everybody is just as capable of

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[1] Bliss, "Researches on Reaction-time and Attention." Studies from the Yale Psych. Laboratory, 1893, p. 3.

[2] Scripture and Moore, "A new Reaction-key and the Time of Voluntary Movements." Stud. Yale Laboratory, 1893, p. 88.

conducting a psychological investigation as the most experienced psychologist. Aside from the lack of technical knowledge and the ignorance in methods of research (research being a distinct art that has to be learned), there is the total lack of training as observers.

4. Errors of statement. There is a fourth source of error of which I am almost ashamed to speak publicly. Psychologists are constantly making measurements and giving the results in figures in a way utterly regardless of the existence of a science of measurement with definite rules. I will not detain you with any remarks on the subject. To careful workers acquainted with the subject, anything I could say would be quite superfluous; to others, anything I could say would also be quite superfluous, although for a different reason.

SOME PSYCHOLOGICAL ILLUSTRATIONS OF THE THEOREMS OF BERNOULLI AND POISSON.

By E. W. Scripture, Ph. D., Yale University.

Professor Bruns (*Ueber die Ausgleichung statistischer Zählungen in der Psychophysik*, *Phil. Stud.*, 1893, IX. p. 1) has pointed out the loss of energy in psychology consequent on the ignorance of the past development of the exact sciences. It is my desire in the present article to show how we can distinguish good psychological work from indifferent and bad by employing means known to every mathematician, but generally unknown to psychologists. The fact that I make use of recent publications by Professor Münsterberg for this purpose is not in any way to be taken as a disparagement of his work. The statements made in his "Studies from the Harvard Psychological Laboratory" in the first number of the *Psychological Review* for 1894, led me to inquire how far the conclusions drawn were justified by the facts given. As he has not verified his results by the proper mathematical means, the labor fell upon his readers.

In the chapter on Optical Time-Content, a series of conclusions is drawn in regard to the overestimation of an interval of time filled with one kind of visual impression as compared with an interval filled with another kind. The first question to ask is this: Accepting the facts exactly as stated, how much reliance is to be placed on the conclusions drawn from them? For all statistical work a definite answer was given by Jacob Bernoulli. Briefly stated, it is this: If there are n total cases in which A occurs r times and is absent s times, then the certainty that the observed result r agrees with the true result within the limits

$$l = \pm \gamma \sqrt{\frac{2rs}{n}}$$

is given by

$$P = \frac{2}{\sqrt{\pi}} \int_0^{\gamma} e^{-t^2} dt + \frac{\sqrt{n}\, e^{-\gamma^2}}{\sqrt{2\pi rs}}$$

In case III. there are results given for six persons in their overestimations of a changing color as compared with a monotonous one in 100 experiments each. The arithmetical mean of all the results is fifty-nine, but the range of variation extends from fifty-one for J. to sixty-five for P. The average range of variation is thus, $l = 7$. With what confidence can we state that the actual average fifty-nine expresses the truth? Substituting the values, $l = 7$, $r = 59$, $s = 41$, $n = 6$, in the above equations, we find that $P = 24\,\%$. Or, in other words, we would be justified in expecting that once out of every

four times a result lying somewhere between fifty-one and sixty-five would be obtained, which—it must be confessed—is a very small amount of trustworthiness in scientific work.

What should Professor Münsterberg have done? The answer is given by a reversal of Bayes's theorem, which I believe I am justified in making. It can be stated as follows: If the probability of the occurrence of an event is p, and of its non-occurrence is q, then, in order to assign a limit, l, with a certainty of

$$P = \frac{2}{\sqrt{\pi}} \int_0^{\gamma} e^{-t^2} d\,t$$

the total number of cases must be $n = \frac{2\,\gamma^2\,p\,q}{l^2}$. As the experiments were made in hundreds, the numbers fifty-one, fifty-nine and sixty-five are also percentages. Thus $p = 59\,\%$ and $q = 41\,\%$. In statistical work we generally demand a degree of certainty.

$$P = 0.999978$$

on a scale of 1 = infinite or absolute certainty. For this value, $\gamma = 3$. By carrying out the processes indicated, we find that $n = 872$. From which we must conclude that with so inaccurate a method and with such variation among the individuals, a conclusion is justifiable only on the basis of experiments on nearly 900 individuals. Every statistician knows that experiments on a few individuals are of no value unless there is practically no variation among them. On the basis of six individuals, Professor Münsterberg says: "It appears to me unquestionably that, etc." Case III. is, moreover, flatly contradicted by case II., where the individuals disagree completely.

A very pretty illustration of one of Poisson's theorems can be drawn from Professor Münsterberg's statements on "Memory," p. 36. Two series, each of 2,140 presentations, visible and audible, were compared as to the frequency of mistakes. How large must be the difference between the results, in order to justify the conclusion that they are really different? The theorem can be stated thus: If in two groups, n and n^1, the observed frequencies are p, q, p^1, q^1, then we can conclude with a certainty of

$$P = \frac{2}{\sqrt{\pi}} \int_0^{\gamma} e^{-t^2} d\,t$$

that the difference between the true probabilities R, R^1, will be

$$K = R - R^1 = p - p^1 \pm \gamma \sqrt{\frac{2\,p\,q}{n} + \frac{2\,p^1\,q^1}{n^1}}$$

Let us take the most unfavorable case, W., which gives 25.1 % for the visual series and 31.6 % for the auditory. Here $n = n^1 = 2{,}140$, $p = 0.251$, $q = 0.749$, $p^1 = 0.316$, $q^1 = 0.684$, and, with a demanded certainty of $P = 0.999978$, $\gamma = 3$. The computation gives 0.058 for the limits within which the observed difference may vary from the true difference. The observed difference is $31.6 - 25.1 = 0.065$, which exceeds the limits of possible variation. Professor Münsterberg is thus justified in concluding that the visual memory excels the auditory memory under the given circumstances.

THE RELATION OF THE INTERFERENCE TO THE PRACTICE EFFECT OF AN ASSOCIATION.

By JOHN A. BERGSTRÖM.

The present paper gives the result of a further study of memory by the method described by the writer in this JOURNAL, Vol. V. No. 3. While the problem studied here has a close connection with that of the previous paper, it has been in part suggested by Dr. Münsterberg's "Gedächtnisstudien," Part I., *Beiträge*, Heft 4, to which reference was made before, and by experiment X. of Müller and Schumann's recent studies, "Experimentelle Beiträge zur Untersuchung des Gedächtnisses," (*Zeitschr. f. Psych. u. Phys. der Sinnesorgane*, Bd. VI., 2 and 3, p. 173, 1893.)

The statement of Dr. Münsterberg's problem is this: Can a given association function automatically while some effect of a previous and different association with the same stimulus remains? He concludes that it can, and that nerve currents do not divide like electric currents along different lines of association inversely as the resistance, but that a slight difference, one way or another, diverts the whole discharge in that direction. The experiments, as may be remembered, were made with some of the common habits of daily life, such as the opening of the door of his room, dipping the pen in ink, and taking the watch out of the pocket.

The statement of Müller and Schumann's problem is this: When a series of nonsense syllables has been learned till the first correct repetition is possible, and is then relearned to the same extent after a certain interval, will more repetitions be required if, in the meantime, the syllables of the series have been associated with another set of syllables? Two twelve-syllable series were learned at a given hour — one for a test, the other for a comparison experiment. The syllables of the test series were then united with twelve new syllables and made up into two series of such a nature that each syllable of the test series should enter into an association with a new syllable.

These were then learned. Two hours from the beginning of the experiment, the original test and comparison series were relearned. Schumann required for the first learning of the test and comparison series 13.1 and 13.0 repetitions, for the relearning 7.29 and 7.89, respectively. As the authors point out, the effect of the interference of associations is neither demonstrated nor disproved, since there is a source of error in the experiment. A considerable part of the work of learning a nonsense syllable series is spent in learning the syllables as such, and in this case the syllables of the test series are repeated about twice as many times as those of the comparison series before the final relearning of the two. The influence of the resulting greater familiarity with the syllables is, perhaps, consider-

able, and all that can be said is that the interference effect is not greater than this practice effect. There is also the fact that the new syllables contain some of the elements of those that were displaced.

The authors, however, suggest another reason, namely, that associative interference is a small and transient influence, and that so few repetitions only are needed to make the first association act to the exclusion of the other that the interference effect, if any, is not great enough to appear, and that Dr. Münsterberg's conclusion referred to above may here find additional evidence. The impressions of their subjects, however, favor the opinion that the interference of associations has considerable influence, especially at first in learning new series.

The experiments reported in two brief articles upon this subject by the writer, demonstrate the fact of interference and show the rate of its decrease with time, and that it is nearly a constant quantity after a little practice. In the case of these experiments we are, however, dealing with the formation of a new association to take the place of a previous one, and not with the revival of a still earlier one, so that they do not exclude the hypothesis that in the case of two associations connected with one stimulus, a slight superiority of one makes it appropriate the whole nervous discharge.

The common view is, perhaps, that a nervous excitation radiates in all directions with an intensity proportional to the permeability, which is in turn, within certain limits, proportional to the number of repetitions; and that the work of breaking up a habit is roughly proportional to the work of forming it. It is hard to say, from general considerations, what should be expected in this case. Interference, as an obstruction to association, is often injurious to the individual, and we might expect that some means of obviating the difficulty should have been developed. On the other hand, it makes for the stability of the associational system of the brain — the very great degree of which is usually not realized in view of the great variety and apparently erratic nature of the mental processes. Interference may, on the other hand, be due to a fundamental mechanical condition, over which natural selection can have no influence, and might, therefore, be expected to appear in simple cases, while in the more complex cases, means of avoiding it may have been developed.

These special questions may all be grouped under the more general problem of the relation of the interference to the practice effect of an association. To the solution of this question in its simplest cases the experiments of this paper aim to contribute. Four cases are possible. The interference effect may be greater than the practice effect; it may be equal to it, or less than it; or the relation of the two may be a variable one.

The experiments were made during March and April, 1894. The subject, M. E. B., was wholly ignorant of the object of the experiments during the entire time, and had no special interest in the matter, except to make as quick records as possible. M. E. B. had become very expert from practice in previous experiments. There had, however, been no practice for about a year, and the records at first are about seven per cent. longer than those of the year before, or those made at the end of the present series. All the records made are used in the tables.

For a fuller description of the experiments, and the precautions necessary, the reader is referred to the previous paper (this JOURNAL, Vol. V. No. 3). The experiment consists of sorting eighty cards into ten different piles, each to contain eight cards with the

same picture. In sorting the same pack a second time, a given card may be placed in the position which it originally occupied or in one of nine other positions. If the piles, in the second trial, are in the same positions as in the first, we have a simple practice effect; if in one of the nine other positions, the pictures on the cards enter into associations which necessarily exclude their former associations and we have an interference effect. The first table gives the result of a preliminary experiment, the plan of which will be seen to be like that of experiment X. of Müller and Schumann's studies. The conditions here are, however, much more simple. The influence of changing familiarity with the objects to be associated is minimal in this case, since the outline pictures used have become perfectly well known by long practice; and any given picture is associated first with one movement or position and then with another, and forms only to a small extent a member of a series. Let A_1 and A_2 denote two entirely different arrangements of the piles into which the same pack is to be sorted; also, let B_1 and B_2 be different arrangements of another pack, and D an entirely different pack. The numbers at the upper right hand corner indicate the number or order of the repetition. Each experiment consists of two parts, a test and a comparison set of records, and may be represented as follows:

I.		II.	
A_1 ——	A_2 —— A_1^2	B_1 — D ——	B_1^2

The interval from A_1 to A_1^2, and from B_1 to B_1^2, is 315 seconds. A_2 and D were begun 120 seconds after A_1 and B_1 respectively. The length of time required for A_2 and D is such as to make the interval between A_2 and A_1^2, and D and B_1^2, also approximately 120 seconds (105-112). The packs represented by D had words printed at the top instead of outline pictures. These are about as difficult to sort as A_2, and were used to make the amount and distribution of the work done in the interval of II. approximately equal to that done in the interval of I., so that no difference of nervous excitement or fatigue might disturb the results. After a little practice the variations of the experiment are very small. This is probably due to the uniform attention which can be given to it, since the conditions are natural and the work is rather pleasantly exciting. The material is also quite regular.

There are twenty-four records for each value given in Table I., that is, 144 in all. The middle record is given as the most probable value, rather than the arithmetical average. What corresponds to the probable error of the average was obtained by taking the differences between the middle record of the whole and the middle records of the upper and lower halves respectively and dividing by $\sqrt{24}$. The middle record of the whole is, of course, the average of the 12th and 13th records. The distribution of errors is quite asym-

TABLE I.

	A_1	A_2	A_1^2	B_1	D	B_1^2
Middle record	60.5	83.1	75.6	60.2	90	52.7
Range of variation	+1.15, —.7	+.95,—1.42	+1.25,—1.52	+.84,—.88	+1.88,—1.8	+1.14, —.95

metrical in A_1, A_2, A_1^2, and B_1^2, but in the reverse direction in A_2 and A_1^2 from that which we usually expect in psychological measurements.

The interference effect of A_2 upon A_1^2 will be found by taking the difference between A_1^2 and B_1^2, the time which would have been required by A_1^2 if A_2 had not been sorted. This is 22.9 seconds, which is very nearly equal to the interference effect of A_1 upon A_2, —22.6 seconds. The effect is evidently quite marked and there is not a single negative result, either in these or the subsequent records. We may make three hypotheses with regard to the nature of the process: first, that A_1 and A_2 efface each other and that consequently the sorting of A_1^2 takes place under nearly the same conditions as A_1, and hence requires less time than A_2; or, secondly, that the practice effect is not effaced, but remains unchanged as a tendency, and that the difference between A_2 and A_1^2 is simply due to the practice effect which remains over from A_1. Thirdly, we might have partial effacement and partial permanence of the individual associations. If the first hypothesis is true, then a totally different arrangement, A_3, should have the same advantage as A_1^2. If the third is true, A_3 would require less time than if there were no effacement. Evidence in connection with Table II. will be given to show that the second hypothesis is the true one. General considerations also support this view. Any given idea enters into a great variety of combinations, and if its different associations tended to efface each other, we could not recall at will the different associates. As has been pointed out before, we have difficulty in these cases, due to the interference of association, and must often wait till the strength of the interfering association diminishes before we can get the one we want; but the fact that we can get it at all shows that it was not effaced.

This preliminary experiment does not, however, give us the relation between the interference and practice effect even as far as it goes; and a much greater number of repetitions of each association is desirable. As far as we know yet, practice may be more or less permanent and have a greater or less influence than interference. Further, the relative value of the divisions within the range of variation of the experiment ought to be known, or something like a zero method adopted for studying the subject. To meet these requirements the following experiment, which is the one upon which this discussion is based, was made. Using the same notation as before, the plan of the first part of the experiment may be represented as follows:

$$A_1^1\ A_2^1\ A_1^2\ A_2^2\ A_1^3\ A_2^3 \ldots\ldots\ldots\ldots A_1^8\ A_2^8$$

and that of the second, or comparison part, by

$$A_1^1\ B_1^1\ A_1^2\ B_1^2\ A_1^3\ B_1^3 \ldots\ldots\ldots\ldots A_1^8\ B_1^8.$$

A pack was sorted every three minutes, so that each part of the experiment required forty-eight minutes. Consider any two corresponding records in part I., as (A_1^6 and A_2^6). The memory work done upon each association of A_1^6, before it is itself sorted, is 5×8, or forty practice associations, and also forty interference associations. The interference associations have an advantage in being a *little* more recent. We know, from the experiments of the previous paper, approximately the rate of decrease of interference at this stage of practice for this subject. The difference between the interference for a two and an eight minute interval is only 3.49 seconds. On the whole, we should expect the accumulated advantage of the interference associations to come within five seconds. Accordingly, if the practice and interference effects of an association are equivalent, we should expect that A_1^6 will require from one to five seconds

longer than A_1^1, which we find to be the case, if we take as a more probable value of A_1^1, 61, the average of 58.53 and 63.48, or if we take the middle records. Similar reasoning applies to all the members of the A_1 series. A_2^6, on the other hand, has been preceded by forty practice associations and forty-eight interference associations. As regards time, the interference associations have the same advantage as in the case of A_1^6. We should, accordingly, expect to find the records of the A_2 series a little longer than those of the A_1 series.

All the precautions found necessary in the experiments of the previous paper have been observed here, and in addition, since it was necessary to use the same cards twice in these experiments and the number of repetitions was so great, an interval of a couple of weeks was observed before the same pack was used again. The arithmetical average and the usual probable error are given in Tables II. and III., since the number of records seemed too small for the same treatment as those in table I. Each average is the average of eight records. The middle records of the A_1 series, that is, the average of the fourth and fifth records, are also given, since this series is especially important.

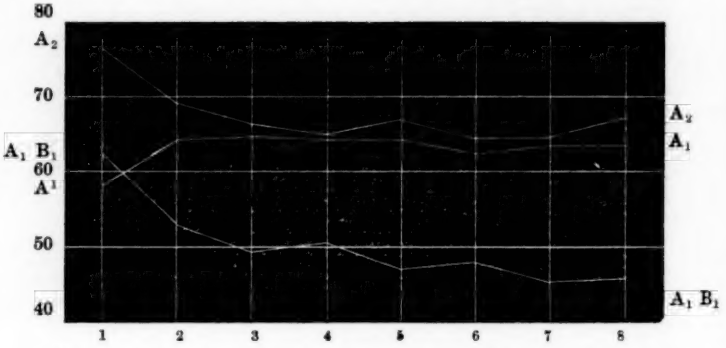
TABLE II.

No. of packs sorted.	1	2	3	4	5	6	7	8
A_2 Series	76.63	69.15	66.3	64.53	66.61	64.5	64.1	67.08
P. E. Av.	±1.55	± .728	± .95	±1.03	± .982	±1.087	±1.37	±1.22
A_1 Series	58.53	64.95	65.30	64.05	63.98	62.68	63.85	63.35
P. E. Av.	± .807	± .989	±1.47	±1.38	± .906	±1.00	± .713	±1.36
A_1—B_1 Series	63.48	52.85	49.86	50.6	47.2	47.95	45.52	45.52
P. E. Av.	±1.27	±1.35	±1.07	±1.06	± .893	± .969	± .129	±1.1
Middle Records of A_1 Series	59.4	63.7	62.7	63.1	63.3	62.4	63.	62.4

The records of the A_1 and B_1 sets of the comparison experiment A_1 B_1, correspond closely and are united in the averages.

The first record of the A_1 series is smaller; and that of the A_1 B_1 series is greater than we should expect at this stage of practice. The average of the two, 61 seconds, is probably more nearly correct. The first middle record of the A_1 series (59.4 seconds) is, contrary to the usual rule, larger than the arithmetical mean, which indi-

cates that this mean is too small. The middle records of the A_1 series are, with this exception, smaller than the corresponding averages. They are quite uniform and are perhaps to be preferred.



The facts of Table II. may be seen more easily in the accompanying cut. $A_2\ A_2$, $A_1\ A_1$, and $A_1\ B_1$—$A_1\ B_1$ are the curves which correspond respectively to the A_2, A_1, and $A_1\ B_1$ averages of the table. The abscissas represent the number of repetitions of each association, which varied from one to sixty-four. They may also represent the number of packs sorted, *i. e.*, from one to eight, and, since this was done at regular intervals, they may also represent the time. If we select any point along the abscissas, as, for example, the last, we see at once that under approximately the same conditions the preceding fifty-six practice associations reduce the record to 45.52, and that fifty-six practice and fifty-six interference associations so balance each other that the record is very nearly what it would be if they had not been made. It is, of course, a little higher, but this can be explained by the fact that the interference associations are a little more recent. The total interference effect of the sixty-four associations, as we know from eight tests taken at the end of the $A_1\ B_1$ series, makes the record 83.4 seconds. In general, we may say that the line $A_1\ A_1$ would be above or below its present position if the interference effect were not approximately equivalent to the practice effect, but either greater or less. If we assume that the practice effect is proportional to the number of repetitions, and that the interference effect is equal to the practice effect, multiplied by a certain constant, greater or less than unity, the line $A_1\ A_1$ would evidently be straight, though making a certain angle with its present position. With the same assumption regarding practice and the number of repetitions, if the interference effect sustains a variable relation to the practice effect, $A_1\ A_1$ would evidently be curved. If, on the other hand, the practice effect is not directly proportional to the number of repetitions, but is some other function of it, the last two assumptions about the relation of interference to practice being maintained, $A_1\ A_1$ would have a curvature respectively similar to or different from that of the true practice curve. The true practice curve may, of course, differ considerably from the usual practice curve obtained by experiment, since the mechanical conditions of the experiment may prevent any reduction in time and yet the result of practice

show itself by a greater permanence of the associations. The true practice and interference curve should represent the energy of the associations, which is here the thing considered. If we take the interference records of our experiment as even approximately proportional to the true interference effect, it is evident that the first eight repetitions have relatively much greater influence upon the associative tendency than the subsequent fifty-six. Whatever the relation between true practice and the number of repetitions may be, the fact that A_1 A_1 is nearly parallel to the axis of abscissas, and is nearly straight from 2 to 8, and is, besides, but slightly above the level of records which have been preceded by neither practice nor interference associations, can only be explained by assuming the equivalence of the practice and interference effects. With the assumption of a variable relation of interference to practice, we might explain the fact that A_1 A_1 is straight and parallel to the axis of abscissas on any assumption of the relation of practice to the number of repetitions, but not its present position.

The total number of repetitions is about twelve times that required for the first free repetition from memory, since the cards can be thrown without reference to the piles when about two-thirds of the first pack has been sorted. The result is accordingly true for quite an advanced stage of practice.

There are, however, certain sources of error in the experiment which should be mentioned. With continued work most individuals experience at first an exaltation, then a depression of power. With the present distribution of the work, and this stage of practice, the variations for M. E. B. from these causes are very small, if demonstrable at all. In a previous experiment, tests at the beginning and end of an hour's work gave the records 57.05 and 56.30 respectively. The average of twelve hours, in which the kind and distribution of the work were very much like that of experiment II., does not show any such fluctuations. With two exceptions, the experiments of Table II., as well as those just referred to, were made about 8 A. M. The other two were made at what the subject felt were favorable times in the afternoon. All that is important is to take a time in which the subject's energy is not liable to variation.

The question regarding the nature of the nervous process, discussed in connection with experiment I., can now be answered definitely. If the series A_1 A_1 and A_2 A_2 do not simply balance but actually efface each other, any other arrangement of the same cards A_3, would have the same advantage as A_1^8 or A_2^8. At the end of four experiments a test was made with A_3. The average of the records is 91.6 seconds, showing a great amount of interference. Hence, while different associations with the same stimulus interfere, they do not efface each other, but retain an individual existence. They probably do not even partially efface each other, as is shown by the following fact: The total interference effect of the sixty-four repetitions of the A_1 B_1 series gives a record of 83.4 seconds as an average of eight experiments. The tests by which the interference is shown were made as regular continuations of the experiment. While considerable variation must be allowed in the case of 91.6 seconds, it does not seem probable that the interference of the A_1 A_2 series is any less than that of the A_1 B_1 series, as would be expected if the contradictory associations partially effaced each other. This last matter is, however, not quite certain. In the A_1 A_2 series there are two associations to interfere; in A_1 B_1 only one. Furthermore, a little more work is done upon the A_1 A_2 series, the result of which we may, perhaps, suppose would be

to make the associations A_1 and A_2 stronger than they would be without interference. These facts leave room for the possibility of a small partial effacement. The results of previous experiments, on the other hand, support the view that the interference effect of two or more associations is not greater than that of one, other things being equal; since, if a succession of different arrangements, A_1, A_2, A_3, A_4, etc., are used, the time of A_2 is considerably longer than that of A_1, but the time of A_3, A_4, etc., is nearly the same as A_2, and the fact that the interference is greater in the A_1 A_2 than in the A_1 B_1 series might meet the demands of the second consideration, if they are just.[1] It seems probable, therefore, that there is not even a partial effacement of the associations.

In previous experiments the amount of interference remained quite constant, even while there was considerable change in general practice. We may explain this by assuming that the practice effect for that particular part of the process represented by the interfering associations attained its full measure very soon. While there is no great difference in general practice noticeable between experiments I. and II., there is considerable with regard to the interference effect. This may be due to the greater complexity of this part of the process in these experiments. It even seems probable that if experiment II. had been made at the stage of practice of experiment I., the records would have shown that the interference effect is greater than the practice effect. The test of the matter is of course to be made when the subject is in good training and capable of taking advantage of or mastering the practice and interference tendencies with something like uniform effectiveness. The amount that $A_1{}^2$ is above A_1 especially shows differences in the two groups of experiments gathered up in Tables I. and II., being 15.1 seconds in the first as compared with 3.95 seconds, or at most, 6.45 seconds, if we take the average of A_1, Table II., at 58.53 seconds. A third group of experiments was accordingly made exactly like the first. The result is given in Table III. There are ten records for each average.

TABLE III.

	A_1	A_2	$A_1{}^2$	B_1	D	$B_1{}^2$
	56.80	72.32	63.28	55.37	82.06	47.06
P. E. Av.	± .987	±1.1	± .807	± .930	±1.28	± .589

The intervals are not quite the same, but the difference is not such that we can expect much change in the results from this source as compared with Table II. The close correspondence with Table II. is sufficiently evident. Here, as in Table I., A_2—A_1 very nearly equals $A_1{}^2$—$B_1{}^2$, the differences being 15.52 seconds and 16.22 seconds respectively, showing that the interference effect of A_2 upon $A_1{}^2$ is the same as that of A_1 upon A_2.

For the proper estimation of the results of measurements, we must know the range of variation and the scale of equal values of

[1] AMERICAN JOURNAL OF PSYCHOLOGY, Vol. VI. No. 2, p. 272.

the instrument we employ. The shortest record made in experiment II. in the A_1 B_1 series is 36.6 and the longest, with this series, that is, in the interference experiment, 101 seconds. The difference of the average of these interference tests, 83.4 seconds, and the last average of A_1 B_1, 45.52 seconds, may be called the average range of this experiment. This is not, however, the maximum average range, since the minimum record suggests the possibility of a still lower average, and we have an interference average of 91.6 seconds. The lower limit is fixed by the maximum rapidity of handling the cards and the process of discrimination and choice. The upper limit is not so well defined and probably exceeds the time which would be required if the memory of the positions did not co-operate at all—which would seem a natural limit—since the subject looks in the wrong direction more than he would if the matter were left to chance.

To get some estimate of the value of the divisions of the range of variation, we may ask: What is the variation which equivalent tendencies of association produce above and below sixty-one seconds?—the record made when no previous associations are present. The total practice effect of the first fifty-six associations of the A_1 B_1 series reduced the record to 45.52 seconds. From the general course of the curve we can assume with only a small error that the effect of sixty-four associations would have been the same. The total interference effect of sixty-four associations gives an average record of 83.4 seconds. The difference above sixty-one is 22.4 seconds, below 15.52 seconds for these equal forces. To get a comparison of smaller variations, let us assume that the records of the first paper[1] are comparable with those of experiments II. The justification for this is that the stage of general practice is about the same, and especially that the interference with the 120 seconds interval is nearly the same in both cases—namely, 13.57 seconds and 15.63 seconds. The practice effect of eight associations, with a 300 seconds interval, is 8.15 seconds. The interference effect (from Table III. of the above-mentioned paper) is about twelve seconds. The ratio $\frac{8.15}{12}$ is nearly equal to $\frac{15.52}{22.4}$. Accordingly, as measured by the variations in the experiment, the practice effect is two-thirds the interference effect; although, as measured by a method in which there was only a slight change in the records, they are equivalent. The latter is of course the true measure and shows that the variations above are relatively greater for the same associative tendency, in these special cases, in the ratio of three to two. These records are not sufficient to enable us to map out the scale more in detail.

To summarize the results briefly: We have shown that under the simple conditions of this experiment, the interference effect of an association bears a constant relation to the practice effect, and is, in fact, equivalent to it. As regards the nature of the process, it has been shown that we are dealing with the interference of still persisting associations and not with the results of their effacement.

To estimate the results correctly we must refer to the conditions of the experiment. The cards are sorted with the greatest possible speed. Every assistance the memory of the positions can give is utilized. All conditions, except the memory factor, are approximately the same in each test; and the variations in the records are due to the latter. As has been stated before, psychophysic processes, higher than simple successive association, seem to be

[1] American Journal of Psychology, Vol. V. p. 361.

excluded. If at the end of the A_1 A_2 series of tests M. E. B. tries to recall the positions of both A_1 and A_2, this can usually be done, though with difficulty. Considerable time would be required to fix the twenty positions in memory, especially since they are contradictory. The associations are here in balance, and in sorting with the greatest possible speed, it is nearly the same as if there had been no previous associations; nevertheless, if the subject takes time to think, every card can usually be placed correctly, from memory. This is another proof that the associations persist and have not been effaced, and also that in sorting with the greatest possible speed, higher processes are excluded, but that such may modify the result greatly if the work is done deliberately.

Since we know what the relation of practice to interference is in a simple case, we have the basis for understanding the changes of the results when other factors enter.

The nervous system has, as is well known, inherited tendencies of growth and adjustment to external circumstances. Perhaps the simplest of these, for the organization of nervous activity, is the tendency of nerve currents to run from one impression to the next succeeding. This is modified in many ways by special tendencies of higher order, which may be classed as fundamental practical adjustments, or practical interests. If these are given an opportunity to influence the results, we are not dealing with associations *per se*, but with these as modified by other more powerful forces. These means of avoiding interference, however, require time and energy. More time is required, otherwise we might expect them to assist us when we act with the greatest possible haste. More energy is required, since the process is more complex and the work of analysis, which is the incorporation of parts of one process in another, is proverbially difficult. There are, besides, great numbers of secondary tendencies by which simple successive association is transformed. Moreover, where the conditions are more complex than in this experiment, certain elements may enter and change the results. A person who speaks several languages finds that the words of the same language tend to be recalled together. Some persons, also, know and can use two different systems of shorthand. In these cases the elements associated are used as members of a group, for the exclusive employment of which there seems to be a strong tendency. The inconvenience of interference is thus to some extent avoided in these cases, but probably only by proportionate expenditure of energy.

The theory as to the character of nervous associations, referred to in the beginning, namely, that a nervous discharge does not radiate into all lines of association, but is wholly appropriated by the association which has a momentary advantage, is evidently not supported by the results of these experiments. The fact that the approximate proportionality of the interference and practice effects of an association can be demonstrated in any case in which simple successive association is utilized to the full extent, leads us to suspect that where facts seem to correspond with this theory, some accessory associations have been developed to bring about the result.

In conclusion I wish to acknowledge my indebtedness to Dr. Sanford for valuable suggestions and to my wife, who has greatly aided me in this study.

PSYCHOLOGICAL LITERATURE.

I.—NEUROLOGY.

By C. F. Hodge, Ph. D.

Brain Surgery. M. Allen Starr. New York, 1893; pp. 295, fifty-nine Illustrations.

The primary aim of this book, as the title indicates, is to give the surgical status of the subject of brain localization. In addition to this, it may serve to furnish an expression of what has come to be generally accepted in this country in this much controverted subject.

Chapter first treats of "The diagnosis of cerebral disease." Here we learn that "there are certain areas upon the cortex of the brain, not necessarily co-extensive with either lobes or convolutions, whose functions are accurately known." Of these the author would make five, viz.: (1) The sensori-motor area; (2) The speech areas; (3) The visual area; (4) The auditory area; (5) The areas of the sensations of smell and taste. Each of these is briefly outlined with the aid of the clearest possible diagrams. With regard to the "sensori-motor" areas, the view is taken of foci with wide intermerging areas of representation. "Each motion, each part of a limb, has a wide general representation over the cortex and a special representation at a limited area." "The areas of representation of the different limbs merge into one another." "This explains the fact that excision of a small area does not totally paralyze the portion of the limb chiefly represented on that area. The adjacent areas represent to some extent that limb and hence can govern it if need be." The author reasserts his view, as the designation of this region indicates, that there is some loss of tactile sensation as well as paralysis when a lesion of the motor areas occurs. Speech areas, motor, auditory, and visual, are located, in the left hemisphere for right-handed persons, in the posterior part of the third frontal convolution, in the first and second temporal convolutions, and in the lower parietal angular region respectively. The location of graphic speech, the power of writing, is not definitely determined, cases on record pointing to both the second frontal convolution and the lower parietal near the hand centre. In regard to the visual centre the author follows Henschen in placing the primary centre in the calcarine fissure. Audition is located in the first and second temporal convolutions, each ear being connected with both hemispheres, so that total deafness is never caused by unilateral lesion. Taste and smell occupy the tip of the temporal lobe, lower and median surface. Like the centres for hearing, each lobe supplies the sense organs of both sides, so that unilateral lesion rarely produces noticeable symptoms. The

frontal region, the great terra incognita, has been in an uncer tain way irresistibly associated with higher psychic functions. The author's experience seems to support this view. Lesions of this region cause no disturbance of motion, of sensation, or of speech. "Yet for the coördination of facts into orderly series, for comparison, and for analysis of knowledge gained through the senses, the healthy state of the frontal lobes appears to be necessary. And lesions in the frontal region, especially upon the left side, are quite uniformly attended by mental dullness, apathy, lack of concentration and imperfect self-control." The functions of the basal ganglia, optic thalami and corpora striata, are still undetermined. If a lesion here does not invade the internal capsule, its presence can not be detected during life. The cerebellum Starr considers as the organ for control of bodily equilibrium.

In Chapter II. pp. 19-113, we have discussed at length trephining for epilepsy. Cases are clearly stated, and supplementing this a microscopical study of two of the cases by Van Gieson is given. The net result of operations reported is ten cases cured; six, improved; eleven, not improved; and two fatal. Trephining for imbecility due to microcephalus, Chap. III., 114-156, is an operation where the surgeon encounters great odds. Out of thirty-four cases, fourteen died, and five showed no improvement; while eight were somewhat, and seven greatly, improved. When gross atrophies are present, surgical interference is of no avail; but when brain tissue has been arrested in its development by cysts, clots, or tumors, or by early union of sutures, the removal of these disturbing conditions by giving more space may result in stimulating growth and in improvement or cure. Chapters follow upon trephining for cerebral hemorrhage, for abscess and for tumor of the brain. In this last field Starr has added to his 300 cases of brain tumor in patients under twenty years, 300 from adults; and his table combining the two lists of cases shows some striking facts touching the relative frequency of various kinds of tumor at different ages. Tumors of tubercular origin are more than three times (152 to 41) as frequent in children as in adults. Also tumors of the cerebellum, pons and medulla are more than twice as numerous in children. Of the whole 600 tumors, for reasons of character and position, only forty-six were clearly open to operation and only thirty-seven, about six per cent., could probably have been successfully removed. Trephining for insanity, Chap. VIII., has proved of service in the rare cases, about two per cent., where insanity has developed immediately after serious injury to the head. It has also been tried, both in this country and in England, for the relief of general paresis, but without effect, or accompanied with only such temporary improvement as may occur in any case of this disease.

Trephining for headache, Chap. XIX., has been successful in two cases, in both of which the disease was of traumatic origin and sharply localized. The book closes with a chapter on the operation of trephining, which is of purely technical interest.

The Sensory Motor Functions of the Central Convolutions of the Cortex Cerebri. F. W. MOTT. Brit. Med. J., II. pp. 685-7. 1893.

The point of this research consists in the very satisfactory evidence which it brings to bear upon the sensory functions of the so-called motor areas. The work was done chiefly on monkeys, portions of the brain being removed with a Horsely brain knife. Definite regions were paralyzed in this way and after determining these, sensibility was tested by spring clips of different strengths

applied to the skin. The animals reacted uniformly, always removing the clips from the side unaffected by the lesion in the brain, and in general not noticing them on the paralyzed side. The natural explanation is, of course, that the animal is unconscious of sensation upon that side. A more detailed account of these experiments may be looked for in an early number of the *Journal of Physiology*.

Preliminary Observations on Some Changes Caused in Nervous Tissues by Reagents Commonly Used to Harden Them. HENRY H. DONALDSON. Jour. of Morphology, Vol. IX., pp. 123-166. Boston, 1894.

This paper casts a shade of doubt over the records of brain weights as they are usually accepted. Unless it is known exactly how any particular brain has been treated before weighing, the weight as recorded may be anywhere between thirty per cent. too large and thirty per cent. too small. In general, bi-chromate of potash solutions swell, while alcohol has a tendency to shrink the brain, and these processes may even pass beyond the limits indicated above. A large number of experiments were made on sheep's brains in a number of different solutions, and the general reactions recorded. To have the research cover as much of the field as possible, these were then repeated upon sharks' brains and upon a series of human brains. Results were in all cases entirely similar. All possible variations of temperature, strength of solution, manner of cutting the brain, degree of dryness, drainage, age of individual, length of time post mortem, etc., were taken into careful account, so that with these data at hand, it is now possible to correct the weight of any given brain to its original weight when fresh.

In the gross changes, however, we have but a small part of the value of this research. The brain swells or shrinks on account of changes taking place in its tissue elements, the nerve cells. The sizes of these may, therefore, be far from normal, as given by the text-books. For consideration of this side of the subject, we must await a subsequent chapter. The paper should be in the hands of everyone who is contributing to neurological science. We confess to some disappointment in not finding an explicit set of directions for obtaining the most nearly correct weight of the brain possible, the outline of a method which would unify and make comparable the work of different observers. Gathering some such statement from the article, we should say that the brain should be weighed fresh, as it comes from the skull, with pia intact. A note should record whether the olfactory bulbs and pituitary body have been retained, and describe where the division between medulla and spinal cord has been made. The state of the blood vessels should also be described. If immediate weighing is not possible, careful note should be taken of all treatment to which it is subjected up to the time at which it is weighed.

An enormous amount of work has been condensed into little more than twenty pages by stating nearly all the results in tabular form. The tables, forty-eight in number, give the briefest and clearest statement of the case possible, and make the data easily accessible for reference.

Brain Preservation, with a Résumé of Some Old and New Methods. PIERRE A. FISH. The Wilder Quarter-Century Book, pp. 385-400, 1 Plate.

This will be found a convenient compendium of some of the better methods of brain preservation, chiefly with reference to

gross specimens. Contributions to the subject have not yet resulted, according to the author, in discovering the ideal method of preparation; but to one who has himself experimented along this line, this will not appear as any unpardonable failure. A number of experiments have been made, and as a result, a new fluid is recommended. Its composition is water and alcohol, each 400 c.c.; glycerine, 250 c.c.; zinc chloride and sodium chloride, 20 gm. each. The specimen, after having its ventricles, and, if possible, its blood vessels, injected with this fluid, is immersed in it for three days, and subsequently in a mixture of equal parts of the fluid and seventy per cent. alcohol for a week or more, when it is finally preserved in ninety per cent. alcohol. Further experiments are under way, which we may hope to hear from later.

The Effect of Stimulation and of Changes in Temperature upon the Irritability and Conductivity of Nerve Fibers. W. H. HOWELL, S. P. BUDGETT AND ED. LEONARD. Journal of Physiology, Vol. XVI., pp. 298-319. London, 1894.

The purpose of this research, somewhat different from similar studies in this field in which attention has centred about irritability of nerve fibers at different temperatures, is to discover the relations of temperature to conductivity, with the hope of throwing some light on the nature of the nerve impulse. By the use of both medullated and non-medullated nerves, it was hoped to learn something also in regard to the function of the medullary sheath.

The method consisted in cutting the nerve and laying it in a small brass tube, around which water of any desired temperature was made to circulate. Experiments were made on both frogs and mammals, rabbit, dog and cat. It has been known for some time that nerves may be warmed or cooled within certain limits to a point where they lose their irritability and conductivity, and that, on bringing them back to a temperature between these limits, they may regain both these properties. The principal additions to our knowledge contributed by the present investigation, refer to the order in which conductivity is arrested by cooling in different classes of nerve fibers, and to something unique in nerve-fiber-physiology, viz., a rapid fatigue of certain fibers at the point of stimulation, hence called by the authors, "stimulation fatigue." Since this is a point by itself, it may be considered first. "Stimulation fatigue" was discovered in connection with stimulation of the sciatic nerve of the cat for the study of the effects of cooling on motor fibers, vaso-constrictor fibers, and secretory fibers to the sweat glands of the paw. In every case, whether cold was applied or not, stimulation ceased to have any effect on the constrictor and secretory fibers, and it looked as though a genuine nerve-fiber-fatigue effect had been demonstrated. Later, it was found that this fatigue existed only at the point of stimulation, and that if the electrodes were shifted a mm. down the nerve, no similar fatigue of motor fibers was present, and since these are medullated and the vascular and secretory are non-medullated, it seemed to point to functional difference due to the medullary sheath. To test this point, stimulation was applied to the rami between the spinal cord and sympathetic ganglia, where both secretory and vaso-constrictor nerves are supposed to possess medullary sheaths. Here the results crossed, the vaso-constrictor fibers being not subject to stimulation fatigue, as predicted, while the secretory fibers were fatigued as before. This breaks down the interesting generalization and leads to the conclusion that the phenomenon of stimula-

tion fatigue must be due to differences in the conducting portion of the fibers, rather than in any difference in their sheaths.

Several other points of interest may be abbreviated from the conclusions as follows: 1. Cooling may be conveniently used to block the nerve impulse where it is desirable to suspend conductivity without injury to the nerve. 2. The temperature at which conductivity is suspended varies somewhat in different fibers, lying between 5 and 0 C. The cardiac inhibitory fibers of the rabbit offer an exception to both of the above rules in not regaining conductivity well and in losing it at 15 C. 3. "A nerve impulse in passing into a stretch of fiber of different temperature may suffer an increase or a diminution in force, according as the temperature of this portion of the nerve is above or below that in which the impulse originated." The force of the impulse is increased by heat and diminished by cold. 4. The method of cooling may be used to differentiate the physiological varieties of nerve fibers combined in a common trunk, viz., to separate vaso-constrictors from vaso-dilators in the same trunk, inhibitory and augmentory fibers in the vagus, etc.

A Microscopical Study of Changes Due to Functional Activity in Nerve Cells. C. F. HODGE. Journal of Morphology. Vol. VII. pp. 95-168. Plates VII. and VIII. 1892.

The earlier experiments in this research were first reported in this JOURNAL for 1888, '89 and '91, and dealt respectively with the changes produced in spinal ganglion cells by electrical stimulation and with the process of recovery from fatigue thus produced. It is unnecessary to recapitulate the results of these experiments further than to remind the reader that the nucleus became smaller, irregular in outline and stained darker as stimulation was continued, and the cell protoplasm became more or less vacuolated according to the degree of fatigue induced.

The point in which the present paper forms an advance is in a study by similar methods of effects in the nerve cells of normal daily activity, and it is intended in this review to cover this last section of the work.

The experiments were made by taking the animals, English sparrow, pigeon, swallow and honey bee, at the beginning and end of their day's work. The above animals were chosen because of their constant and well defined rhythm of diurnal activity. Similar preparations of the cerebrum, cerebellum and spinal ganglia were compared in six pairs, morning and night, of birds, and the cerebral ganglia of ten couples of bees each morning and night. The result, which is of greatest interest to psychologists, is that a greater degree of fatigue-change is often produced by ordinary daily work than can be obtained by electrical stimulation. Sets of cells were measured as in the former experiments, and the nuclei were found in all cases smaller in the evening specimens. This difference in the spinal ganglion cells of the birds amounted to from thirty-three to sixty-four per cent., showing an average for the birds of forty-eight and two-tenths per cent. The nuclei of the cells of the occipital cortex showed a slightly greater difference, thirty-six to sixty-nine and seven-tenths per cent., with an average loss of fifty-one and five-tenths per cent. In the honey bee experiments, the nuclei of the antennary lobes were measured and showed a shrinkage in volume of from nine to seventy-five per cent. The spinal ganglia of two foxes were also examined and only a moderate degree of change was demonstrated. As the carcasses after skinning could not be identified, the amount of fatigue, or the length of

chase, could not be determined. In the motor cells of the spinal cord of a patient dying of hydrophobia, the nuclei were found to be much shrunken, being nine per cent. smaller than corresponding nuclei in a so-called normal human cord. It is not strange that we should find a greater amount of change in daily fatigue than in artificial stimulation. It is not possible to obtain secretion of a gland or contraction of a muscle by application of electrical stimuli equal to that produced by the normal nerve impulse to gland or muscle. Two plates containing thirteen figures give the entire research at a glance.

The Formation of the Medullary Groove and Some Other Features of Embryonic Development in the Elasmobranchs. WILLIAM A. LOCY. Jour. Morph., Vol. VIII. 1893.

The Optic Vesicles of Elasmobranchs and their Serial Relation to Other Structures on the Cephalic Plate. Ibid., Vol. IX., pp. 115-122. 1894.

Metameric Segmentation in the Medullary Folds and Embryonic Rim. (Prelim. Comm.) Anatomischer Anzeiger, IX., pp. 393-415. 1894.

Locy states at the close of the last paper that he is not ready, as yet, to generalize upon the segmentation of the vertebrate nervous system. However, the facts which he brings together point strongly toward a helpful generalization in the near future. It has come to be quite generally held by morphologists that it is the mesoblast which becomes segmented primarily, and that segmentation of the neural tube is moulded by these bone and muscle somites and accommodates itself to them. Contrary to this view, in embryonic stages much too young to show any trace of mesoblastic somites, Locy finds a perfectly regular, symmetrical and constant segmentation of the neural plate. He has succeeded, in one of the sharks, in tracing out this segmentation in a consecutive and orderly way, and has also succeeded in demonstrating it in early embryos of Amblystoma, Diemyctylus and Torpedo Ocellata. In all Locy finds eleven metameres in the expanded portion of the neural plate which represents the brain. These are distributed to the three primitive cerebral vesicles as follows: six for the third vesicle, two for the second, and three for the first. Later all traces of this segmentation become masked by the development of special structures throughout this entire region. The fact of such a segmentation appearing so early should be given prominence in working out the ancestry of vertebrates. At first all the metameres are alike, which would indicate an ancestral form of this character, *i. e.*, without differentiation in the neural tube. As cephalization advances, differentiation takes place, and it is in this that the primitive segmentation is lost.

This brings us to the first pages of our author. The first structures to make their appearance in the segmented neural plate are the pits which represent the optic vesicles. Just behind these appear a second pair of depressions, the so-called accessory optic vesicles, which give rise later to the pineal gland. Still a third pair of pits may be observed behind the second, but these early become obscured. This series of depressions is taken to represent a multiple eyed condition, common enough in invertebrates, but not known in any vertebrate, and this, too, is of significance in a search after the ancestral form. The arrangement of these optic vesicles in a laterally symmetrical series inclines the author to the view that the eye may be homologized with the sense organs which spring from the lateral line.

The Epiphysis of Teliosts and Amia. CHARLES HILL. Jour. Morph. Vol. IX., pp. 237-266. Pls. XII. and XIII. 1894.

The special significance of this paper consists in its filling a gap in our knowledge of the pineal structures in the bony fishes. In connection also with Locy's observations on Elasmobranchs, it is of interest to note that Hill finds two independent outgrowths arising from the roof of the mid brain in the teliosts examined and in amia. These together form the epiphysis, but each vesicle remains distinct. The author considers it probable also that in their primitive position they were side by side and subsequently become crowded into the median line by the growth of the hemispheres. The plates, twenty-two figures, give the clearest possible account of the subject.

Comparative Study of the Epiphysis and Roof of the Diencephalon. A. D. SORENSON. Jour. Comp. Neurol., Vol. IV., pp. 12-72. 1894.

This paper gives the literature of the pineal region more in extenso than any that we have. Each author is treated separately and the main points have been gathered together, summaries copied, etc., so as to give the organ as it has been described in the different classes of vertebrates.

Treatment and Prophylaxis of Insanity. JOHN PUNTON, M. D. Alienist and Neurologist, Vol. XV., pp. 52-66. St. Louis, 1894.

The particular form of mental disease treated by our author is melancholia, the most common and most easily managed of insanities. It may arise as a congenital neurosis or be acquired. Primarily, according to Meynert and Clouston, melancholia arises from trophic disturbances, malnutrition, of the cortex. This theory the author permits to dominate his treatment. Insomnia is a frequent symptom, but drugs which have a tendency to interfere with nutrition, opium and the bromides, are contra-indicated. Sulphonal is the best remedy to apply in these cases. Everything must be done to bring up body weight, generally deficient in melancholiacs, and force nutritive processes to the utmost. For this purpose quinine, strychnia, phosphorus, arsenic, cod-liver oil, mineral acids, vegetable bitters, hypophosphates, et al., and especially foods, milk and eggs: "Three quarts of milk a day and six eggs for months." Yet any treatment will fail without daily exercise in the open air. The chief aim of the paper is to direct the attention of the medical profession to the importance of preventive measures while there is possibility of cure. If this in the case of congenital defects be begun at birth and even before, and continued through nursery and school life, tendencies of this character may be eradicated. An index of especial value in cases tending toward melancholia is body weight. Patients should weigh at least once a month, and if any loss is detected, they should adopt measures immediately to make it up and keep it up.

The Value of Sugar, and the Effect of Smoking on Muscular Work. VAUGHAN HARLEY. Journal of Physiology, Vol. XVI., pp. 97-122, London, 1894.

The experiments were made in Mosso's laboratory with the ergograph. Nasse, Brücke and Weiss have shown that glycogen in the muscles decreases in amount during activity and accumulates during rest. And more recent researches of Chauveau and Kaufmann have demonstrated that sugar in the blood disappears much more rapidly when circulating in an active than in a resting muscle.

With this as a basis, the author attempts to decide the practical value of sugar as an energy-producing food. He finds in using the ergograph that he fatigues much as Lombard does, viz., with periods of partial recovery. He can thus use the point of first fatigue as a measure of the condition of his muscles. As another measure he uses the total amount of work accomplished by the first thirty contractions. Diet was carefully regulated, and the experiments were repeated daily at the same hours. As in all former work in this line, practice was found to increase strength and regular diurnal variations occur. The total amount of work on a sugar diet is almost equal to that on a full diet, although fatigue occurs sooner. Days of fast compared with days on which 500 gms. of sugar were taken, showed an increase in amount of work for the sugar days of from sixty-one to seventy-six per cent. The effect of sugar added to a light meal increases work from six to thirty-nine per cent.; and even when added to a heavy meal, gives an increase of from two to seven per cent. Smoking has a much slighter effect on Harley than on Lombard, causing a diminution in work scarcely demonstrable.

Neue Darstellung vom histologischen Bau des Centralnervensystems. RAMON Y CAJAL. Archiv für Anatomie und Entwickelungsgeschichte, pp. 319-428, 35 Figures in text. Leipzig, 1893.

In the above form we are presented with a translation from the Spanish, to which numerous revisions and additions, both in text and figures, have been made by Cajal himself. It furnishes a clear connected statement of this successful investigator's views and researches up to date, and in addition to this a running critical review of the work of others. Specifying, in a single instance, we note that Cajal denominates Dogiel's position as to anastomosis of cell processes as heretical, "ketzerisch." But the good reasons which he is able to advance remove from the sentence all taint of the Spanish inquisition. The spinal cord, cerebellum, cerebrum, different regions, retina and olfactory bulb are treated in order. No brief review of this important paper can be attempted.

Report in Pathology (upon gross and microscopical lesions found in thirty-eight cases, autopsies of insane patients). T. P. PROUT. Annual Reports of the New Jersey State Hospitals, 1893, pp. 99-117.

Dr. Prout is resident pathologist in the Morris Plains Asylum, and is to be commended upon the industry and patience with which he has dealt with his difficult problems. Gross lesions, either in the brain or its membranes and blood supply, are demonstrable in all cases. Among these, thinning of cortex and atrophy of convolutions with diminished or increased consistency of brain substance, anæmia, and œdematous and opaque condition of the pia, are especially frequent. Nearly all the cases were chronic, over half being terminal dementia (thirteen) and general paralysis (seven). Concerning his microscopical findings, Dr. Prout says: "The changes in the cells in all these cases were, it seems to me, ample to account for the mental disturbance manifested, provided we may consider the cortical cell the seat of mental activity, a point quite generally conceded." Degenerations of all sorts, fatty, pigmentary and granular, vacuolation of cell protoplasm and especially of nucleus, with fragmentation of the nucleus, are described in some detail and are figured. It is probably to be put to the account of Dr. Prout's equipment that no brain weights appear

in his excellent tabular statements of cases. With the apparatus supplied to state institutions, it is useless to insist that *weight of body, weight of brain* and *stature* should be given in case of every autopsy, but no really available data can be obtained for this country until this is done. For anyone who may wish to repeat and confirm Prout's observations, more exact statement as to the location examined will be necessary, and some indication of the methods employed. Definiteness upon these points would have added greatly to the practical value of the work.

Der Einfluss des Trigeminus auf die Hornhaut. J. GAULE. Centralblatt f. Physiol., Bd. V., pp. 409-15. 1892.

Wie beherrsht der Trigeminus die Ernährung der Hornhaut. Ibid., pp. 450-56.

Spinalganglien und Haut. Ibid., Bd. V., pp. 689-97.

Spinalganglien des Kaninchens. Ibid., Bd. VI., pp. 313-26.

Weitere Experimente an den Spinalganglien und hinteren Wurzeln. Ibid., Bd. VI., pp. 785-802. 1893.

Die trophischen Veränderungen und die Muskelzerreissungen. Ibid., Bd. VII., pp. 646-54. 1894.

Die trophischen Eigenschaften der Nerven. Berliner klin. Wochenschr., Vol. XXX., pp. 1065-68 and 1099-1102.

Zur Frage über die trophischen Functionen des Trigeminus. C. ECHARD. Centralbl. f. Physiol., Bd. VI., pp. 328-32. 1892.

Ueber das Vorkommen von Muskelzerreissungen an gefesselten Kaninchen. H. E. HERING. Ibid., Bd. VII., No. 18.

Erwiderung auf Herrn Prof. Gaule's Bemerkungen über die bei gefesselten Kaninchen vorkommenden Muskelzerreissungen. H. E. HERING. Ibid., Bd. VIII., pp. 854-7.

The above group of papers may be cited as an interesting contribution to the subject of the trophic influence of the nervous system. For several years Gaule has been working to bring some physiological explanation to bear upon the array of facts, like that of herpes zoster, decubitus, atrophy of glands and muscles, after their nerves are severed, and many others, which seem to indicate clearly a trophic action of the nerves. He begins with the cornea, where effects may be most clearly observed, and performing Majendie's experiment on the fifth nerve and Gasserian ganglion, makes out definite changes in the cornea, drying and necrosis of the epithelium cells, wholly dependent on the operation and which cannot be thwarted by any possible protection of the surface. The fifth nerve was cut in different experiments at three points, through the Gasserian ganglion, between pons and ganglion and between cornea and ganglion. The result upon the cornea did not follow when the cut was made between pons and ganglion, and this at once proves that insensibility of the cornea cannot be the cause of its becoming dry, and in consequence, necrotic; but that necrosis of the cells is the cause of its drying, and hence that the cells of the Gasserian ganglion are true trophic centers for the cornea. Thus the usual explanation is exactly reversed. Similar observations were made upon the skin in frogs and rabbits, the hair interfering with entirely satisfactory study of the latter. In the frogs, destruction of the spinal ganglia was found to cause changes com-

parable with those described for the cornea, with certain differences in the dorsal and ventral regions. These consisted in a drying of the dorsal parts and a corresponding swelling with turgescence of the ventral. Beside changes in skin and cornea, injury to the spinal ganglia produces most unaccountable alterations in many of the deeper organs. The adrenal bodies show constant change, and the reproductive gland of the opposite side becomes shrunken and of especial interest; the muscles become the seat of active changes. It is to these that the author devotes chief attention in his later papers. They consist in hemorrhages, varying in size from that of a ten cent piece or larger to that of a millet seed, into the substance of the muscle or into the connective tissue septa. At first they seemed to occur throughout the entire muscular system, in greater or less numbers, out of all observable connection with known anatomical relations, "ganz unberechenbar" as to location. Later Gaule succeeded in demonstrating certain anatomical paths for two muscles, the biceps and psoas. The effect could be made to appear in the muscles by a number of operations upon the spinal ganglia, by stimulating with the interruption of a strong (four to five Daniel cells) constant current (an induction current was not effectual), and by cutting or cauterizing the ganglion. These must all be done without previously opening the sac or disturbing the blood supply of the ganglion.

Echard maintains that such flaws in the cornea, as Gaule describes in his first papers, are readily produced in rabbits by a little rough handling, a little dust, hair chippings and the like getting into the eyes, even if no operation whatever is made, or if the fifth nerve be cut close to the pons. Gaule answers this criticism by calling attention to the fact that the root of the fifth nerve contains many ganglion cells at its emergence from the pons. He also brings forward an experiment of crucial importance. If the Gasserian ganglion be cut upon one side and the root of the fifth nerve be cut upon the other side above the ganglion, where no cells occur, the cornea will degenerate upon the side of injury to the ganglion, and this cannot be prevented by care and protection, whereas the cornea on the other side remains normal. Here we have a satisfactory answer given to the old argument against trophic influence of nerves based upon insensibility and consequent neglect of parts concerned, both corneæ being, in this case, equally insensible.

Hering's work, done and reported apparently without reference to Gaule's experiments, goes to prove that hemorrhages in the muscles may be produced in rabbits by their voluntary efforts to escape, when simply tied in the manner usual for operation. These hemorrhages closely resemble those described by Gaule as trophic, and occur most frequently in the biceps and psoas, exactly the muscles most clearly affected in Gaule's experiments.

This brings us to a consideration of Gaule's work upon the sympathetic ganglia. He found that if he stimulated or injured the superior sympathetic ganglion, changes invariably took place in the biceps and psoas muscles. The path of this influence, according to the author's view, must lie in the spinal cord and must reach this through the ramus communicans. From stimulation upon one side, the muscles of both sides are affected. Further, a peculiar complication exists in the fact that one branch of the superior cervical ganglion acts to inhibit all trophic influence of the ganglion upon the above named muscles. If this twig is stimulated, stimulation of the ganglion at the same time produces no effect. So definite is the reaction that Gaule is able to lay bare the living biceps and upon stimulation of the ganglion, actually observe the

whole process. As stimulation of the ganglion proceeds, the muscle, at about the middle, near the entrance of the nerve, first becomes rough and irregularly contracted, then œdematous. The muscle fibers apparently dissolve and tear apart under the strain of normal tonus; the capillaries, unable to support the strain, are torn, thus producing the hemorrhage, and in from three to ten minutes may be seen the formation of a typical ulcer. After the operation this heals slowly, with formation of a scar.

Thus Gaule is able to prove, as he thinks, the absence of all connection between his experiments and those of Hering. It is further stated that the rabbits experimented on are not tied down, but held by the hand, and that rupture of the muscle fibers may occur without perceptible voluntary contractions. The essential fact, upon which Gaule insists, is that trophic changes may be produced in muscle substance by which its power to resist strain is greatly diminished. He experiments further to prove this by extending similar muscles with different weights. Normally, a weight of 5,000 gms. does not cause rupture of muscle fibers, while 500 gms. does so invariably, if the spinal ganglia are injured. Thus resistance is lessened to at least one-tenth of normal.

We can no longer, according to Gaule, speak of "trophic nerves." The whole nervous system is trophic.

II.—ANTHROPOLOGICAL PSYCHOLOGY.

A. F. CHAMBERLAIN, PH. D.

LINGUISTICS (and related subjects).

Further Notes on Indian Child-Language. A. F. CHAMBERLAIN. *Amer. Anthrop.* (Washington), VI. (1893), 321-322.

Notes that it is the mothers who teach the children many of these words.

Secret Language of Children. O. CHRISMAN. *Science* (New York), XXII. (1893), 303-305.

Treats of the secret language in use among school-children and others, and contains original observations on the Tut-language, as spoken by the children in Gonzales, Texas.

On the Words "Anahuac" and "Nahuatl." D. G. BRINTON. *Amer. Antiq.* (Chicago), XV. (1893), 377-382.

An interesting onomatological study. The author brings out the curious fact that in the Zapotec Indian tongue of Mexico (as also in Huastec), "the verb 'to know' is a reduplication of the first person of the personal pronoun *na*, 'I;' *na-na*, 'to know,' literally, 'my mine,' that which is with me, essentially mine."

The Origin of Literary Forms. C. LETOURNEAU. *Pop. Sci. Mo.* (New York), XLIII. (1893), 673-682.

Treats of the forerunners and the beginnings of literary æsthetics and their progress through the various periods of human development. Refers to the Fuegians, ancient Greeks and Latins, Chinese. The author considers "that there are good grounds for supposing that women may have especially participated in the creation of this lyric of the erotic kind."

A New Method of Teaching Language. W. VIETOR. *Educ. Rev.* (New York), VI. (1893), 350-359.

In this paper Professor Vietor, of the University of Marburg, gives an account of the new or reform method of direct or imitative language teaching as practiced in many of the German secondary schools, with a brief sketch of a reform lesson.

Reform in Modern Language Instruction. J. J. FINDLAY. *Ibid.*, V. 334-344.

Discusses Gouin's system chiefly.

Relations of Literature and Philology. O. F. EMERSON. *Ibid.*, V. (1892-'3), 130-141.

Argues for the complete separation of the study of two such diverse subjects as in accord with the specializing tendencies of the age, and with the necessities of academic progress.

La Degenerazione nello stile dei paranoici erotici. G. C. FERRARI. Rivista Sperim. di Fren. e di Med. Leg. ecc. (Reggio), Vol. XIX. (1893), 329-363.

Follows up the study of Tanzi, who sought to show the analogies between the writings of paranoics and those of primitive peoples. A detailed account of observed "degeneration in the style of paranoics," illustrated by numerous citations in prose and in verse, from the original documents. The anomalies are considered atavistic returns due to the fundamentally degenerated condition of the patient.

Ueber eine Depressionsform der Intelligenz in sprachlicher Beziehung. M. O. FRAENKEL. Ztschr. f. Psych. u. Phys. d. Sinnesorgane (Hamburg-Leipzig), VI. (1893), 230-232.

Treats of peculiarities (the use of weak forms of strong verbs, *gegehen* for *gegangen*, *mir* for *mich*, and *vice versa*, etc.) observed in the speech of a twenty-three year old (seemingly idiotic) woman. Notes the fact that in French, which she speaks like her mother tongue, the same mistakes are not made.

The Language Used to Domestic Animals. W. H. CARRUTH. Dialect Notes (Cambridge, Mass.), Part VI. (1893), 263-268.

Discusses the peculiar words in calling, driving away, etc., cows, swine, horses, sheep, dogs, fowls, in various districts of the United States, giving phonetic transcriptions of each sound noticed. An interesting beginning of an important study.

On the Source of the Italian and English Idioms, Meaning "To Take Time by the Forelock," with Special Reference to Bojardo's Orlando Innamorato. Book II. Cantos VII.-IX. G. E. MATZKE. Public. Mod. Lang. Ass. Amer. (Baltimore), VIII. N. S. I. (1893), 303-304.

An interesting psycho-linguistic study. The paper is supplemented by others by Professor G. L. Kittredge, Mod. Lang. Notes (Baltimore), VIII. (1893), 459-469; and Dr. K. Pietsch, *Ibid.*, 469-475.

The Order of Words in Anglo-Saxon Prose. C. A. SMITH. Public. Mod. Lang. Ass. Amer. (1893), 210-244.

A timely consideration of a much neglected subject. The author discusses the influence of Latin, and later of Norman-French, inversion, transposition, etc. His conclusion is, "The leading dif-

ference between the word order in Anglo-Saxon and that in Middle English or Modern English, is found in the frequent transposition occurring in Anglo-Saxon dependent clauses," and the introduction of Norman-French only consummated the influences at work to produce this.

The Absolute Participle in Middle and Modern English. C. H. Ross. Ibid., 245-302.

This is a thorough-going and statistical study, and the author's statement that in Modern English the absolute participle "is an important adjunct to the style, to which it imparts variety and compactness. It gives life and movement to the sentence, and is the ready resource of all writers of narration and description for the purpose of expressing subordinate conceptions," ought to lead to the revision of certain text-books of rhetoric and composition. Mr. Ross' part is based upon the examination of twenty-nine Middle English, seven Old French, one Italian, and sixty-one Modern English texts.

A Grouping of Figures of Speech, Based upon the Principle of their Effectiveness. H. E. Greene. Ibid., 432-450.

The author's order is: "Synecdoche, Metonymy, Stated Simile, Implied Simile, Metaphor, Personification, Imperfect Allegory, Pure Allegory. Kenning, which points sometimes toward Metonymy, sometimes towards Metaphor, I place between Metonymy and Metaphor." Professor Greene remarks that "since the publication nearly forty years ago of 'The Philosophy of Style' by Herbert Spencer, there has been a gradual concensus of opinion in favor of the view which he advanced—that the aim of all rhetorical devices is economy of the attention of the reader or hearer."

Polysynthesis in the Languages of the American Indians. J. N. B. Hewitt and J. Owen Dorsey. *Amer. Anthrop.* (Washington), VI. (1893), 381-407.

This is an examination by Mr. Hewitt upon the basis of Iroquoian and Dr. Dorsey upon the basis of Siouan speech of the "polysynthesis," held by many authorities to be characteristic of American tongues and in criticism of views expressed by Dr. D. G. Brinton, upon this question of great psychological import in linguistic study. Of the American Indian tongues it may be said: "They, like the languages of the older hemisphere, have traits which are found in the majority of languages, and they also individually have others which are idiomatic." Nay, more, what Professor Whitney has written about holophrasis in view of the languages of the Indo-European family, applies with equal force to the languages of the American aborigines, the word-sentences of which are the same in kind with those of the former.

On Adaptation of Suffixes in Congeneric Classes of Substantives. M. Bloomfield. *Amer. Jour. of Philol.* (Baltimore), XII. (1891), 1-29.

In this excellent paper, Professor Bloomfield treats a subject of great psychological interest and one with which the students of the languages of our American aborigines are tolerably familiar. The author, however, confines his field of view to Indo-European speech, treating of the suffixes occurring in the names of the body and its members, words for right and left, assimilation of opposites

and assimilation of congeners, suffixes in names of birds, animals and plants in Greek, suffixes in names of divisions of time, etc. The paper is a distinct contribution to linguistic psychology.

The Spelling Reform (Bureau of Education, Circular of Information, No. 8, 1893). F. A. MARCH. (Washington), (1893), 86 pp. 8vo.

This is a revised and enlarged issue of the pamphlet published by the United States Bureau of Education in 1881 and exhibits the progress of the movement for the reform of our absurd system of spelling since that date, progress that must encourage the distinguished advocates of the innovation.

A propos de la réforme orthographique. CH. LEBAIGUE. *Revue Pédagogique* (Paris), Tome XXII. (1893), 213-222.

Discusses the report of the Dictionary Committee of the French Academy. The author finds fault with the committee in one point only—it has favored the hyphen too much.

Une Objection contre la Réforme de l'orthographe: l'étymologie. A. RENARD. *Ibid.*, XXIII. (1893), 36-43.

The author holds that etymology has nothing to do in principle with orthography, and supports his views by numerous well-chosen illustrations.

Simplified Spelling; A Symposium. Amer. Anthrop. (Washington), VI. (1893), 137-206.

In this symposium on the question: "Is simplified spelling feasible as proposed by the English and American philological societies?" the following distinguished gentlemen took part: Professor F. A. March, Dr. W. T. Harris, Dr. Alexander Melville Bell, J. M. Gregory, W. B. Owen, E. T. Peters, C. P. G. Scott, J. C. Pilling, A. R. Spofford, B. E. Smith, Professor W. D. Whitney, Major J. W. Powell. The chief who answered the question in the negative, was Dr. Spofford, who said, "The cardinal error of the spelling reformers is: They forget that the language of a people is an evolution, not a creation." Perhaps the most interesting contribution to the symposium is that of Mr. J. C. Pilling, who gives (pp. 182-186) a brief account of the syllabaries used by missionaries and others to record and to teach Indian languages.

The Native Calendar of Central America and Mexico; A Study in Linguistics and Symbolism. D. G. BRINTON. (Philadelphia), (1893), 59 pp. 8vo.

This new and extended study of the calendar-systems of Central American and Mexican aborigines contains much of psychological interest. The mathematical basis, the day-periods, the solar year, divination by the calender, and calender festivals are briefly referred to, but the main portion of the work (pp. 20-49) is devoted to acute analysis of the day and month names, with a discussion of their symbolism (pp. 49-59), and hieratic significations. According to Dr. Brinton, the philosophic conception, which, without any straining, was intended to be conveyed by the calendar, covering the career of human life from birth till death in old age, was this: "The individual emerges from the womb of his mother, and the parturient waters, as did the earth from the primeval ocean; he receives breath and with it life, which is supported by repose and food. The man reproduces his kind; the woman, at the risk of

death, brings her child into the world. The chase and tilling of the ground are the leading occupations of peace, and he who holds firm through illness, suffering and hardships will gain the prize of life. Having reached the acme of his career, the decline commences. Losses multiply, years increase, and though knowledge and wisdom are augmented, old age comes on apace with failing powers, with vanquished struggles, with sickness and death; until, at last, its course run, its task completed, the soul quits the worn-out body and soars to its natural haven and home, the abode of the sun."

Speech Tones. ALEXANDER MELVILLE BELL. (Washington), (1893), pp. 18.

A paper read before the Modern Language Association of America, December 27, 1893, and devoted to the consideration of tones, accent, characteristic national tones of speech.

Internal Speech and Song. J. M. BALDWIN. Philos. Rev. (Boston, New York, Chicago), II. (1893), 385-407.

Treats of "words in our mind," "tunes in our heads," pitch-recognition, etc. A useful contribution to the literature of the psychology of language. Professor Baldwin makes some new suggestions.

Studies of Animal Speech. E. P. EVANS. Pop. Sci. Mo. (New York), XLIII. (1893), 433-437.

Discusses the investigations and statements of Wenzel (1808), Hanikl (1830-40), Richard (1857), Radeau (1869), Nicaise and Garner.

The Speech of Animals. H. N. LYON. Science (New York), XXII. (1893), 324.

The author describes the "efforts at communication" made by a pet fox squirrel.

Report of Committee on Classification of Methods of Instructing the Deaf. Amer. Ann. of the Deaf, XXXVIII. (1893), 291-414.

An exhaustive treatment by specialists of the question of the instruction of the deaf; treats of sign-language and other matters of psychological and linguistic import, besides the various other methods of language-instruction.

Speech for Deaf Children. L. E. LITTLETON. Pop. Sci. Mo. (New York), XLIV. (1893), 363-372.

A general discussion of the subject.

Manual of Linguistics. A concise account of general and English phonology, with supplementary chapters on kindred topics. By JOHN CLARK. New York, G. P. Putnam's Sons, 1893, pp. 318.

This is not a purely psychological book, but will interest and instruct all interested in the psychology of speech. The author is well read, and writes on the origin of speech and of letters, or sound relations, semi-vowels, spirants, consonants and explosives, analogy, ablaut and accent, Grimm's law, etc., with a wide view of what has been done. The author builds mainly on Brugmann and Sweet, and regards phonology as a very exact science. The horizon is Indo-European. Speech was polyphyletic and not monophyletic, and gesture, and festal excitement aided; imitation may have

been the germ of all. The primitive Aryan talked not roots or sound-norms, but sentence-words; "Speech craft is not yet dead;" the great epoch were metals and smith-lore. Phonic law is universal and suggests lines of research; analogy is constructive and a psychological factor, while phonic law is physiological, so that they complement each other. These are some of the *dictata* that may suggest the quality of the book.

The Early Spread of Religious Ideas, Especially in the Far East. JOSEPH EDKINS. F. H. Revell Co.

"Let us continue to follow the New Testament, the Samaritan Codex, and the Septuagint, in regarding the Book of Genesis as pre-Mosaic and compiled from documents in the age of Moses, under his direction." "But invaluable as it is, the Book of Genesis is in the early part so brief and incomplete that we are obliged to look beyond it for information, and must make search in the Zendavesta, the Vedas, the Chinese sacred books, and in the Buddhist Sutras. If God spake to man by Adam, Enos, Enoch and Noah, as Christians believe He did, the truths and duties He taught must exist in some form in the literature of their descendants, among whom are the nations which possess these sacred books." "The most suitable theory for the investigator to hold is that which represents the early monotheism of Asia, wherever found, in China or in Persia or in western Asia, as resulting from divine revelation aiding the human faculties of conspicuous men." "The revelations made to Moses and the prophets were specially entrusted to the care of the Jews. The monotheisms of China and Persia are a survival of the revelation made to Enoch, Noah and other primeval patriarchs." "Emigrants, when they passed beyond the reach of inspired patriarchs, would easily forget a part and alter another part of the truths taught them in the first ages." "The world was peopled twice. . . . . The traditions of the Chinese classics were to a large extent ante-diluvian. The traditions of Tauist and Buddhist books are post-diluvian." "Why should we necessarily have mythological periods in history?" "Mythology is a morbid growth from philosophy." "This conception of God (the idea of a Trinity) . . . . belongs to the last revelation, long ago made in the plains of Babylonia." "Hence it appears to be perfectly safe teaching that divine light granted to Mesopotamia, as the Book of Genesis tells us, was certainly conveyed in radiating lines from the primeval home of mankind to the farthest borders of Asia, and beyond the sea to America. It seems, then, to be right to represent whatever religious and moral truth we find believed in by the nations of the far east as having been conveyed to them from the earliest home of mankind, and as being derived at first from divine revelation." F. D.

Die Medecin der Naturvölker. DR. MAX BARTELS. Ethnologische Beiträge zur Urgeschichte der Medecin. Leipzig, 1893, pp. 361, 8vo.

In this work Dr. Bartels has done for medicine what Ploss did for the history of woman and the child. All the important aspects of primitive medicine, — disease, physician, diagnosis, medicaments, water-cure, massage, relation of the sick and the well, sympathetic treatment, knowledge and diagnosis of special diseases, special pathology and therapeutics, epidemics, surgery, etc., are dealt with, and the extensive bibliography appended shows that the author has familiarized himself with the more recent researches in

America and elsewhere. Most interesting are the chapters on the "doctors," "medicine-men," "shamans," of various peoples, their status, rights and privileges, training, etc., and the section on diseases ascribed to supernatural causes. Particularly noticeable is the wide-spread belief in the evil nature of the spirits of women who have died unmarried, or in child-bed, these in many cases being regarded as very inimical to young children. Among many tribes the position of shaman is hereditary and it is often held by women; in some parts of Africa twins are usually trained to practice the healing art. In South Australia, a young man becomes a doctor "by seeing the devil," *i. e.*, a spirit imparts to him in a dream the requisite knowledge and authority. Of "heroic treatment," many examples are cited from all over the world. The book is one which will interest alike the psychologist and the student of the history of medicine.

Primitive Music. An Inquiry into the Origin and Development of Music, Songs, Instruments, Dances and Pantomimes of Savage Races. With Musical Examples. R. WALLASCHEK. London, 1893, XI. pp. 326+8, 8vo.

This timely, most interesting and valuable work is another evidence of the importance of ethnology for the science of art and the psychology of æsthetics. As Roskoff showed that even the most barbarous peoples had some germs of religious belief, so Wallaschek declares that "however far we might descend in the order of primitive people, we should probably find no race which did not exhibit, at least, some trace of musical aptitude, and sufficient understanding to turn it to account." The author treats of: General character of the music of primitive races; singers and composers in primitive times; instruments; the basis of our musical system; physical and psychical influence of music; text and music; dance and music; primitive drama and pantomime; origin of music; heredity and development. Following are some of Wallaschek's conclusions: "It is quite as difficult a matter to scientifically establish a pure musical type as the purity of a race" (p. 65). "The hypothesis, however, that savage races had female voices, would be entirely unfounded, although some singular exceptions (Australia, China, etc.) speak in favor of it at first sight" (p. 77). "It is with the vocal organs as with those of hearing; there has been practically no change in historic times at any rate, although to-day we may, perhaps, be better able to realize their capabilities than heretofore" (p. 79). "The difference between people with and without harmonic music is not *a historical, but a racial one*" (p. 144). "Among savages the influence of music is far more distinctly noticeable than among people in a higher state of civilization" (p. 163),—the power of music as solace and curative in affliction and disease is widely recognized. "In the relation of text and music there seems to have been little change from time immemorial" (p. 171). Wallaschek's theory of the origin of music is as follows: "From the character of primitive music, as exhibited by the musical practices of savages, I venture to conclude that the origin of music is to be sought in a general desire for rhythmical exercise, and that the 'time-sense' is the psychical source from which it arises" (p. 294). He rejects Spencer's theory of its origin from the "natural melody of emotional speech," as well as the "bird-song" theory. With reference to heredity and development, the author declares: "I consider it downright impossible that heredity of acquired modifications contributes anything to the development of

music" (p. 268). A bibliography of several hundred titles adds to the usefulness of a book which ought to be in the hands of every student of art and man.

Die Urgeschichte der Familie von Standpunkte der Entwickelungslehre. VON WAGNER. Biologisches Centralblatt, XIV. Bd. (1894), s. 65-71.

This is a review of a special chapter in the recent and important work of H. E. Ziegler, "Die Naturwissenschaft und die sozialdemokratische Theorie, etc. (Stuttgart, 1894),"— a work which may be styled "Principles of Sociology upon the Basis of Natural Science." Ziegler deals at considerable length with the primitive family and criticises sharply the well-known views of Morgan and his school, maintaining that the evidence for the family life of the anthropoids points to monogamy, and laying stress upon the psychological aspect of the question, — the instinctive nature of conjugal love, jealousy, love of parents for their children,— he holds that in all probability, even in primitive times, the family life of man was monogamous. In this he agrees with Westermarck. In zoölogical life Ziegler recognizes three stages of development in the sexual relations: 1. The lowest stage, among the sponges, echinoderms, etc., where the sperm-cells are emptied in the water and wander about seeking the eggs, which are likewise migrating. 2. The "Begattungspaarung," found among many worms, arthropods, molluscs, and among the vertebrates, with fishes, amphibia and reptiles, — where two individuals unite for the purpose of reproduction and soon after separate again. Here not seldom appears a sort of love-play, an instinctive wooing and fleeing, or a caressing which precedes the copulation. After copulation, the care of the offspring devolves upon only one sex, usually the female, more rarely, as is the case with the *Gastrosteus aculeatus* and the *Alytes obstetricans*, the male; the highest stage of the method of sexual reproduction entails permanent pairing and the sharing of both sexes in the bringing up of the young— the typical method among birds and mammals. Although the permanent pairing is not everywhere developed in the same manner, but there is everywhere a *psychical relation* between the paired individuals, recognition, dependence, instinctive impulse (love), jealousy. The sexual relation may be either polygamous or monogamous, and for man a monogamic sexual relation seems primitive and natural.

III. EXPERIMENTAL.

RECENT STUDIES OF AN INTERESTING OPTICAL ILLUSION.

(1) *Optische Urtheilstäuschungen.* MÜLLER-LYER. Du Bois-Reymond's Archiv, 1889, Supplement-Band, 263-270.

(2) *Ueber ein optisches Paradoxon.* BRENTANO. Zeitschrift für Psychologie, III., 1892, 349-358.

(3) *Optische Streitfragen.* LIPPS. *Ibid.*, 493-504.

(4) *Ueber ein optisches Paradoxon (Zweiter Artikel).* BRENTANO. *Ibid.*, V., 1893, 61-82.

(5) *Une nouvelle illusion d'optique.* DELBŒUF. Revue Scientifique, LI., 1893, 237-241.

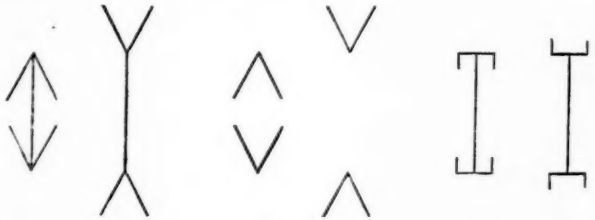
(6) *Les illusions d'optique.* BRUNOT. *Ibid.*, LII., 1893, 210-212.

(7) *Zur Lehre von den optischen Täuschungen.* BRENTANO. Zeitschrift für Psychologie, VI., 1893-94, 1-7.

(8) *Erklärung der Brentano'schen optischen Täuschung.* AUERBACH. *Ibid.*, VII., 1894, 152-160.

Reference may also be made here to the article of JASTROW in this JOURNAL, IV., 381, which contains matter upon this illusion.

The typical form of the illusion that is discussed in the papers above is shown in the first two figures below. The distance between the apexes of the angles in both cases is the same; it appears, however, considerably greater in *B* than in *A*.

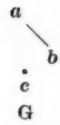


(1) This illusion was first mentioned, so far as the reviewer is aware, by Müller-Lyer, who described it with a number of others of similar character, in 1889, in an article reviewed soon after in this JOURNAL (III., 1890-91, 207). It seems worth while to recall this important paper, as it has been generally overlooked by subsequent writers. Müller-Lyer's explanation, which rests in part upon analogous effects observed in other figures, is that in judging such distances we involuntarily take into account not only the distances themselves, but also a portion of the surrounding spaces also. If, for example, figures *A* and *B* are completed by drawing vertical lines connecting the tips of the short oblique lines, the spaces enclosed would be smaller in *A* than in *B*.

(2) Brentano, without knowing of the work of Müller-Lyer, set about explaining the illusion himself. Of possible explanations he mentions four, but rejects the first three, namely: First, that the small added lines suggest traction and consequently compression in *A* and extension in *B;* second, that the addition of the small lines obscures the termination of the longer ones, which might be expected to result in overestimation in one case and underestimation in the other; and third, that we compare the lines by means of the muscular feelings resulting from running the eye along them and that the small added lines attract attention, which causes the eye to start and stop a little beyond the end of the lines in one case, and a little short of the end in the other. These Brentano rejects for the following reasons: The first, because curves (which do not suggest traction) may be put in place of the short lines without lessening the illusion, and also because the illusion persists, as in *C* and *D*, when there are no central lines to be stretched. The second, because where there are no central lines there can be no question about their termination [a plausible statement, but one that has little force, for an imagined line is almost certainly substituted for the actual one removed—a performance of which Brentano makes much in his

second paper]. The third, because the illusion ought to hold, and does not in figures *E* and *F*. [A remnant of the illusion does remain, as the author recognizes in later papers.]

Brentano's own explanation is based upon the well-known principle that small angles are relatively overestimated. By a series of simplifications of the typical figure, he finally reduces it to a single line and a point, as shown in figure G. The distance *ac* seems a little shorter than it actually is, and this is due in Brentano's opinion to an overestimation of the angle *bac* and an underestimation of the angle *abc*, the lines *ac* and *cb* being furnished by imagination. The overestimation and underestimation thus combine to produce a slight illusory rotation of the line *ab* about its middle point, which would in turn produce an apparent shortening of *ac*. This explanation, once admitted, is easily applied to the more elaborate cases.



G

Brentano's article concludes with the presentation of a group of figures devised to illustrate and further substantiate his explanation.

(3) The first half of the paper of Lipps is devoted to a question of after-images and need not be regarded here; the second half is a critique of Brentano's paper just noticed. Lipps urges, among other objections, that the overestimation, which Brentano assumes, ought to produce a bending of the short line and not a rotation of it; that the principle of the overestimation of small angles and the underestimation of large ones is not of universal validity, but depends on the attendant conditions, and that Brentano's appeal to it is, therefore, unjustified; and finally, that certain of Brentano's own figures (not to speak of others) cannot be explained by this principle, even if admitted. In support of these objections Lipps offers two striking figures of his own, in which the apparent reversal of Brentano's principle is very marked.

The explanation of Lipps (based upon principles worked out at length in his "Aesthetische Factoren der Raumanschauung," Helmholtz Festgruss, Hamburg and Leipzig, 1891) is somewhat as follows: The lines of such figures represent ocular movements. When these movements are free and favored, as they are by extending and expanding lines, the distances passed over seem longer; when they are hindered, as happens with lines of contrary direction, the distances seem short. In Lipps' words, "*Solche frei, 'siegreich' aus sich herausgehende Bewegung nun wird überall in ihrem Erfolg, d. h., hinsichtlich der weite des Weges, der durch sie durchmessen wird, überschatzt, die gehemmte überall unterschatzt.*" This explanation is evidently not very different from the third of the explanations rejected by Brentano, allowing for Lipps' somewhat picturesque way of stating it, and there is, perhaps, some justification for a general statement of this kind, but it is to be noticed that in the familiar illusions of filled and open spaces, hindered movements appear to give a lengthening and free movements a shortening.

(4) In his reply, Brentano takes up the objections of Lipps point by point. He first fortifies his general principle that small angles are relatively overestimated and large ones underestimated by citing further instances, insisting, however, on the relative character of such judgments and on the terms small and large instead of acute and obtuse; of two angles both of which are acute and both are overestimated absolutely, the larger may be less overestimated than the smaller, and so relatively underestimated. He admits readily enough that. like other psychological "laws," this principle

is dependent for its effect in part, at least, on other conditions, but holds that it is sufficiently general for the use which he makes of it. He is wholly successful in explaining the first of Lipps' figures, but (in the reviewer's opinion) only partially so with the second. His replies to Lipps' criticism of some of the figures from his first paper, while apt in the main, do not in every case carry full conviction that he himself is right. Some slight remnants of illusion persist occasionally, and these, he is forced to admit, may have another cause.

Brentano in his counter critique of the explanation of Lipps, denies the special feelings which the latter finds in connection with extending and contracting lines, and holds that if the short oblique lines suggest motion, as they might from their resemblance to arrow heads, it is motion in the contrary direction to that conceived by Lipps — motion outward in *A* above, and inward in *B*. On this supposition, he presents a number of figures, in which an effort is made to strengthen or weaken the illusion by drawing the arrows more fully, but with practically no effect upon it. [This point seems to the reviewer of little significance, for Lipps can certainly reply that the movement suggested by the picture of an arrow is not at all the same as that suggested by a free line; the two belong to totally different psychical levels.] Other figures in which the central line is extended beyond the summit of the angle in both *A* and *B*, with no weakening of the effect or even with a strengthening of it, are much more to the point. A final set attempts to place in opposition the principle of Lipps (in the case in which Brentano admits it) and the principle of the over and underestimation of angles. The figures show a decided triumph of the latter.

(5) Delbœuf was also moved to reply to Brentano's first paper. After giving a careful summary of it, reproducing all the important figures, he adds a number of figures of his own, showing the same or a similar illusion, but in a way not to be explained by Brentano's principle, nor, indeed, by that of Lipps. One of the most interesting of these is one in which the distances to be compared are marked by dots, and the direction of the oblique lines, which in this case do not touch the dots at all, is wholly changed without destroying the illusion. From these he goes on to develop a set of figures of which the type is shown in the diagram below. In this figure the distance from the right edge of the left ring to the left edge of the middle ring is exactly equal to the distance from that point to the right edge of the right ring.

Delbœuf's explanation is essentially the third of those rejected in Brentano's first paper, and not different in the main from that of Lipps. Such illusions are due "to the attraction that lines drawn on an even surface exercise upon the eye;" and again "to the attraction that figures, whatever their form, set at the ends of distances to be measured, exercise upon the eye."

(6) Brunot objects to the explanations of both Brentano and Delbœuf. His opinion is that "in order to judge the mean distance of two objects, the eye instinctively takes the distance of the centres of the figures of the two objects." It is easy to see, then, why *B* should seem longer than *A*, and why the middle ring should seem nearer to the right in Delbœuf's figure. By skillful modifications,

the author applies this same principle to a number of standard cases and, in a somewhat similar way, by taking the general direction of motion suggested by the oblique lines in Zöllner's figure, explains that also.

(7) Brentano's third paper is devoted to Delbœuf. He contends that even if Delbœuf's explanation be true, it does not exclude his own, and presents figures designed to show the two principles in opposing action, with the decided triumph of the latter. [Such figures are really inconclusive, for they would, from Delbœuf's point of view, show nothing more than that lines arranged in different ways are different in effect, which every one would admit.] Of much more force is his objection, supported by clever modifications of Delbœuf's diagrams, that Delbœuf's explanation does not fit his own most characteristic figures, but that they are to be explained in a wholly different way. As to this proper way he is in essential agreement with Brunot above. In concluding he admits that this source of illusion may have coöperated in his typical figures (*A* and *B*), but considers it of subordinate influence.

(8) Auerbach also throws over the explanations of both Brentano and Delbœuf and returns to that of Müller-Lyer, though the paper of that author was unknown to him till his own was in type. The illusion is, he says, "a consequence of the influencing of that which one ought to see by what he sees indirectly in addition," that is, in judging the length of the central line, we take into account the spaces (or imagined lines) to the right and left between the arms of the angles. If this is so, certain deductions are possible with reference to the length of arms, size of angles, and character of the lines forming the arms, etc., which the author finds verified by trial. The points giving the distances to be estimated are quite subordinate things in sensation as compared with the arms; they are simply "not present at all as independent points, they exist only as the places where the arms meet." When means are taken to give the points a certain independence (making them larger, or separating the arms a little from them or making the points different in color), the illusion is weakened, because vision (or attention, perhaps) is less inclined to take the side spaces into account. Variations upon the typical figure are given in support of the author's general view. A few quantitative tests were also made.

As residual from these eight papers, we have, besides a number of interesting observations and a large collection of variant figures, four explanations of the typical illusion. Müller-Lyer and Auerbach explain it by an involuntary regarding of the adjacent areas; Brentano chiefly by over and underestimation of angles; Lipps and Delbœuf by a tendency of the eye to overrun or to come short of the movement required to follow the lines with the eye; and Brunot by an established habit of treating figures or parts of figures as wholes and estimating distances from their centres. A still more general grouping is possible, if we assume, as seems probable, that the misjudgment of angles itself depends in the last analysis on eye movements. Müller-Lyer, Auerbach and Brunot depend on a synthetic tendency in vision; Brentano, Lipps and Delbœuf on a motor tendency. It is almost certain that several causes conspire to produce the illusion in question, simple as it appears, but in what degree each is present cannot be told from present information. The answer to that question must await a thorough quantitative study of the subject.

Grössenschätzungen im Gesichtsfeld. von DR. R. FISCHER. Archiv für Ophthalmologie (1891).

The investigation of this subject occupies two articles, one in the first and the other in the third number of the *Archiv*. It represents two different sets of experiments. The first article is devoted to those on the ocular measurement of lines, and the second to the measurement of angles.

In the measurement of lines the experiments were directed in two different ways. The first method was to divide a given line into two equal parts, and the second to make a line equal to a given one. The arrangement for effecting these results consisted of two lines representing a cross, and placed upon a dark background. The intersection of the two lines formed the point of fixation. The whole of the lines forming the cross was at no time visible, but only such portions of them as were to be compared or divided. An apparatus was arranged to mark off the portions desired for this end. The comparisons were made between the upper and lower and the right and left arms of the cross. One arm was given and the effort was to mark off on the other arm a line equal to the given one, and again to divide a given arm into two equal parts. The experiments were conducted first with both eyes and then afterward with the right eye alone.

The experiments comparing the two vertical arms always showed that the lower arm was overestimated. Out of 560 attempts to divide the vertical arm equally, the average error was 6.70 mm. This result, indicating the difference between the two arms, is expressed by the following proportion: Lower arm : Upper arm :: 100 : 106.70. We may say here that the ratio is against the lower arm. In 128 attempts to make the two arms equal, the average error was 3.22 mm. against the lower arm. The proportions could be formulated as before. In comparing the horizontal arms the constant error was very slight, so slight, indeed, that the estimation was almost exactly correct. In 560 trials at halving a horizontal arm, the average error was only 0.79 mm. against the left arm, the proportion between the two arms being: Left : Right :: 100 : 100.79. In 680 attempts to make the two horizontal arms equal, the average error was only 0.73 mm. against the right arm when the given arm was on the left, and 0.43 against the left when the given arm was on the right. The results here seem to show very distinctly a greater accuracy in the estimation of linear magnitudes by the interior and exterior, or horizontal, than by the superior and inferior, or vertical, muscles of the eyes.

In experiments comparing the vertical and horizontal arms of the cross, the lower arms were considerably overestimated. In 960 trials comparing the lower vertical with the horizontal arm, the average error was 14.67 mm. against the lower, and in 800 trials, comparing the upper arm with the horizontal, the average error was 11.66 mm. against the upper vertical.

The experiments in monocular vision were no less interesting. In 950 attempts to make the vertical arms equal, the average error against the lower arm was 5.63 mm., and in the same number of trials to make the horizontal arms equal, the average error was 3.24 mm. against the right arm. The inference in these cases would be much the same as before, namely, the superior accuracy of vision in the horizontal lines. In 280 experiments comparing the lower vertical with the horizontal arms, the average error was 18.19 mm. against the vertical when the left horizontal was taken, and 15.14 mm. when the right was taken. In 280 trials comparing

the upper vertical with the horizontal, the average error was 10.83 mm. against the vertical when the left horizontal was taken, and 7.70 mm. against it when the right was taken. Here, again, the results show the greater overestimation of the lower arm of the cross.

The results at large are very well summarized in the following manner: First, in regard to the correctness of the estimates of magnitudes, the comparisons of the horizontal arms of the cross with each other by both eyes were the only cases approximating accuracy. In the other cases, (1) the lower arm compared with the upper, was uniformly overestimated; (2) the right arm compared with the left was overestimated; (3) in comparisons of the vertical with the horizontal arms, the lower was overestimated more than the upper vertical. Second, the results also show (1) that the estimates follow the psychophysical law, and (2) that the variable error was only half as great in dividing as in comparing lines, a fact which is construed as proving the practicability of the method of the least observable differences.

In the second paper, describing the experiments for comparing and measuring angles, a circle about 36 cm. in diameter was drawn upon a dark background. This circle was divided accurately into definite portions of thirty degrees each, and these marked by threads issuing from the centre and movably fixed in the circumference. The experiments consisted in efforts to divide a given angle in one of the quadrants into equal parts. Assuming the centre of the circle as the point of fixation, the results would show, as in the case of the cross, the capacity of different portions of the eye for judging magnitudes. The mean or average error was assigned in terms of the percentage of the real half angle to be guessed, and marked with a plus or minus sign, according as the eye judged a quantity larger or smaller than the proper one. The first set of experiments was with the right eye, and the second with the left. The results showed perfect similarity between the two eyes, and were summed up by the author in the statement that "when the angle to be bisected was horizontal or approximately so, the upper angles were overestimated, and the lower underestimated." This was true of both halves of the visual field. The experiments represented attempts to halve angles varying from 10 to 150 degrees. Another and distinct set of experiments were efforts to divide 180 degrees equally, beginning at different points in the circumference. The results were practically the same for each eye, and were very striking in one respect. Beginning at zero, which was the terminus of the vertical diameter in the upper half of the circle, and proceeding to the right with every ten degrees as a starting point for the 180 degrees to be divided, the average error was always plus until seventy degrees were reached, when it became minus and remained minus until 150 degrees were reached, when it became plus again up to 180 degrees. Beginning at zero, or 360 degrees, again, and proceeding to the left, the same distances showed similar results. From 360 to 290 degrees, the average error was plus, and then became minus until 200 degrees were reached, when it was plus to 180. This means that the upper quadrants were overestimated in most cases, and the lower quadrants underestimated in most cases. J. H. HYSLOP.

Ueber Fusionsbewegungen der Augen beim Prismaversuche. von ALFRED GRAEFE. Archiv für Ophthalmologie (1891).

The object of the author is to throw light by special experiments on the question whether binocular accommodation is native or em-

pirical, and to test the comparative strength of the two opposite tendencies to fusion, according as the images are homonymous or heteronymous. The experiments were conducted between the limits of the parallel and the convergent position of the eyes. For testing the native and unalterable functions of binocular accommodation, a prism was used in front of one of the eyes, both horizontally and vertically. When the prism was placed horizontally, so as to produce homonymous images, the movements for fusion had to be divergent and the localization was apparently farther off than in reality, and when placed to produce heteronymous images, involving convergent movements for fusion, the localization was nearer than in reality. These facts are taken as indicating a native and fixed function for localization by binocular adjustment. The result was similar for the vertical position of the prism, which had the effect of throwing the images upon different planes in the retina, and there was no appreciable tendency to fusion, even when they could be brought into the median plane. The author, however, found some slight limitations to the absolute fixity of this law. Even in those cases where vertical fusion seemed to take place, there was reason to regard them as abnormal and exceptional. In regard to the comparative strength of the convergent and the parallel movements of the eyes, experiment seemed to show that the convergent were slightly the stronger. J. H. HYSLOP.

Ueber den Einfluss der Geschwindigkeit des Pulses auf die Zeitdauer der Reactionszeit bei Schalleindrücken. VAN BIERVLIET. Wundt's Philos. Studien, X. (1894), 160-167.

Dr. van Biervliet has measured the sensory reaction-times of eleven university students to auditory stimulation and compared these with pulse rates found by careful counting just before taking the reactions. The instrument used was the Hipp chronoscope, regulated at intervals with the new model Leipzig *Control-hammer*. Six of the eleven subjects showed a regular quickening of the reaction-time with acceleration of the pulse. Four others showed something of the same tendency, but failed at the extremes of fast or slow pulse, and one observer exactly reversed the rule. In view of these more or less discordant cases and of the large size of the mean variation (as in all sensory reactions) when compared with the differences to be established, the quickening of the reaction-time with the pulse rate must be regarded as probable rather than proved. No statement is made as to possible changes in pulse rate during the time of taking a series of reactions, nor are the reasons given for the high pulse rates found sufficiently explicit. One would like to know how far the quickening was due to active exercise and how far to excitement, which last has already been shown (this JOURNAL, IV., 524) to quicken both sensory and muscular reaction-times. Possibly these data may be more fully furnished in the report of experiments on reactions to optical and dermal stimuli that is to be furnished later.

Einige Versuche mit der Wunderscheibe. GRÜTZNER. Pflüger's Archiv, LV., 1893, 508-520.

The author first describes two lantern methods for demonstrating stroboscopic phenomena simultaneously to a large company of spectators. The first presents a single figure in motion, the second a full set of figures. For the full description of these, which cannot be described in short space without the diagrams, the reader is

referred to the original. He next describes a method of mixing colors with the stroboscope, which, in principle, is similar to that of the color top, and might occasionally prove useful. Next follows an application of the stroboscope to the demonstration of simultaneous contrast. Suppose, for simplicity, a stroboscope disk with four slits, two of which (lying in the same diameter) are covered with red glass, the other two being left free. Opposite the stroboscopic disk, on the same axis, is placed a white disk carrying two small black circles. When this combination of disks is set in rapid rotation, the observer sees a white or light red field, in which four dots appear to lie, two red brown and two green, the latter colored by contrast. [Such an experiment seems, like many of Hering's, to speak emphatically for the physiological theory of simultaneous contrast.] A fourth experiment of a more psychological character is the following: A series of stroboscopic pictures in black and white, representing boys playing leap-frog, was taken, and, after it had been viewed for a short time with the stroboscope, the pictures representing one boy in the air exactly over the other, were changed by covering the boy in the air with white paper, those showing the initial and concluding stages of the leap remaining unchanged. The alteration, however, made no difference whatever in the conviction of the observer that he actually saw one boy in the air above the other. Indeed, the suggestion involved in the mere beginning of the spring was sufficient to cause the seeing of the whole of it. A very inviting means is thus offered — as the author points out — for the study of suggested illusion and hallucination. [Work along this line has apparently already been undertaken at Cornell; see page 414 of this JOURNAL.]

Pedagogisch-psychometrische Studien. Zwei vorläufige Mitteilungen. DR. ROBERT KELLER in Winterthur. Biol. Centralblatt. Bd. XIV., No. 1, 2 and 9.

The present study was suggested by Mosso's experiments upon the influence of mental work upon the ergographic fatigue curve. If fatigue is due to a general deterioration of the blood, we may expect it to affect the muscles and that we may find a test, and possibly a measure of it, in the ergograph record. The latter may not be without value as a test of mental fatigue, even if this is of a more local character, since we may expect the strength and number of the impulses sent down by the brain to be diminished by it. In fact, may it not be possible to determine by this method the relative difficulty and the best grouping of studies in schools? The experiments are few in number and were all made upon a boy of fourteen.

The aim of the first is to study the effect of rapid reading upon the ergograph record. The general plan of an experiment is about the same throughout. Four ergograph records were taken, with intervals of about half an hour. A part of these intervals was spent in rapid reading, or other fatigue work. An hour's rest was then taken and an exactly similar experiment begun. Two experiments were made in the morning, and one, sometimes two, in the afternoon. The general result is that rapid reading at first increases, then lessens the ergograph record, and that the record after the hour's rest is usually lower than the fourth record. The fact that this order is repeated in successive experiments on the same day is some guarantee for a causal relation between mental work and changes in the ergograph record. It is also in harmony with Mosso's results. Similar experiments with the rapid reading

of Latin give similar variations, only fatigue appears more quickly. The effect of singing was studied in two experiments. In this case the period of increased activity drops out. In the second preliminary communication, the influence of reading numbers is studied. This work seems to lessen all the ergograph records in the first two experiments, but, in a third, to have the same effect as reading. This may be explained as due to the fact that practice diminishes the fatigue resulting from a given amount of work. The most important thing in these articles is the suggested application of this method of studying fatigue to pedagogical problems. J. A. B.

Recherches sur la localisation des sensations tactiles. Par M. VICTOR HENRI. Archives de Physiol. norm. et Path., No. 4, Oct. 1893.

Where one considers the localization of two points, the results of Weber's circles are considered good, but where only one is to be located, M. Henri thinks the best way is to do it on a photograph or by word. The photographic method was chosen. It was found that the error in direction was nearly constant for any single point of contact. The experiments on the dorsal surface of the fingers of the right hand show that there is a general tendency to locate the points of contact too near the end of the finger. At the end of the finger, however, the tendency is in the opposite direction, though the size of the errors is very much less. There are also some other points at the beginning of the second phalanx similar to the end of the finger. The errors in the transverse direction are of significance only in rare cases. If the point is placed on the side of the finger, instead of in the middle, the error is easily recognized. For any one point the error in localization is very limited, but it varies between 10 and 2 mm. for different points touched. The corresponding parts of different fingers do not vary appreciably. The middle of the first and second phalanges shows the greatest errors in localization, namely, 10 and 8 mm. Near the wrinkles (at the joints), which separate the two phalanges, the errors are equal to 4 or 6 mm. On the wrinkles and on the third phalanx their minimal value, which is 2 or 3 mm., is reached. At other points, however, they are even less than this. One subject found difficulty in distinguishing between points on the ring finger and those on the middle finger, though the amount of error in either case was constant. The difficulty disappeared in moving either of the fingers slightly. On the palm of the hand and at the commencement of the arm, the errors were different in direction and extent. The direction of error was always towards the part which separated the lower arm from the wrist, while the amount of error varied between 5 and 40 mm., being least when the point of touch was nearest this part and greatest in the reverse.

The conclusions from the experiments on three subjects are: (1) Where points are touched on the skin, they are located on the photograph at a point which, in relation to the touched point, is almost constant in direction. (2) Almost always the point indicated on the photograph is more near to a certain wrinkle or fold of the skin (*e. g.*, when there is a joint below) than is the point touched. (3) As the point of contact approaches this wrinkle the errors in localization decrease.

This shows us that all the points where we can localize a touched point are included on the inside of a fixed curve, which the point touched, as a rule on the inside of it, does not meet. Hence two elements of the curve can vary, namely, the size of it and also the distance from its centre to the point touched. These two elements

determine our spatial sensibility in the sense of touch. It follows, then, that if the two points in Weber's circles are not touched simultaneously, they are not located the same as if they were. If, also, any point B is located at A, the point A will not be located at B.

A. E. SEGSWORTH.

IV.—MORBID PSYCHOLOGY.

Die Mehrheit geistiges Personlichkeiten in einen Individuum. Eine psychologische Studie. Von DR. S. LANDMANN. Stuttgart, 1894, pp. 186.

The author is a practicing physician, who for forty years has sought to gain a conception of the nature of mental activities, and here gives us his conclusions, which are as interesting as they are new and carefully matured. Binet's theory of *simultaneous* activity of different spheres of consciousness is radically wrong. All facts on which this theory rests may be explained by supposing that the cortical ganglia of the brain can act unconsciously and reflexly when dissociated, as they may be by many causes, from the cells of the cortex which mediate consciousness. After discussing quite a mass of casuistical material concerning catalepsy, somnambulism, suggested acts and hallucinations, amnesia and distraction, anæsthesia, hysteria, etc., the author reaches the following general conclusions: Lethargy and complete hypnosis are to be explained as transient loss of function by all the brain organs. Cataleptic attitudes are the isolated activity of sub-cortical motor centres. Unconscious imitations of movement by cataleptics and hysterical patients with anæsthesia are due to the isolated activity of the sub-cortical visual centre. The unconscious verbal imitations of catalepsy is due to the isolated activity of sub-cortical centres of hearing. Unconscious mimicry by cataleptic and hysterical patients, and the active innervation feelings of anæsthetic hysteria, are due to isolated activity of the sub-cortical centres of feeling. The acts of catalepsy, the suggestions of hypnotism and hysteria, and suggestions of general hallucination tending toward a change of personality, are due to the activity of a larger or smaller group of cells in the cortex, *i. e.*, to the isolated consciousness of a larger or smaller series of connected concepts. Systematic æsthesia and the "rapport" of hypnosis are due to the activity of cortical cells sensitive to an isolated concept feeling, *i. e.*, an isolated self-consciousness. Hysterical anæsthesia and suggested anæsthesia, lameness, amnesia, etc., are due to the shunting out of isolated cortical cells. Post-hypnotic suggestion and apparent multiplicity of psychic existences are due to the activity of isolated cortical cells, together with the simultaneous normal activity of other brain organs. Systematic anæsthesia, negative hallucinations, and the natural anæsthesia of hysteria, are correlated with the inactivity of the cortical cells responding to the action of the concept, along with simultaneous normal activity, *i. e.*, partial self-consciousness. The automatic activities of self-conscious subjects are due to the isolated activity of sub-cortical ganglia, along with the normal activity of other cortical cells. Finally, somnambulism is due to changing activity of various larger or smaller parts of the brain, with complete inactivity of the other parts.

To complete personality, the sub-cortical centres and all the ccortex must act together. The individual who can be consious of all concepts arising within or without, and of all feelings of activity, is a complete psychic personality. More or less

incomplete egos are those who can bring to consciousness only a part of the concepts of feeling, sense and motion. Dessoir's upper and lower consciousness is rejected. Consciousness is always one and one only, however frequently it may change. Janet's distinction between psychic and psychologic is developed so as to suggest that the subject stands over against its own sensation to which it may open or close its eyes.

La Foule Criminelle. Par PAUL VIGNY. Paris, 1892, pp. 185.

This little volume is translated from the Italian, and is called an essay in the larger field of collective psychology. Society might be regarded as a single person who had always existed, and instead of calling the individual a microcosm, M. Vigny urges that the world is a "macanthrope." Social individuality is for him, as for Tarde, the solar microscope of psychology. The old diction, *Senatores boni viri, senatus autem mala bestia*, illustrates how much worse mobs can be than the persons composing it. Legal responsibility for collective crime is a hard problem, never solved by the classic school of penology. Suggestion, imitation, moral contagion have much, but not final, explanatory power. Mobs are media in which the microbe of evil develops further and easier than the microbe of good. The influence of numbers increases the intensity of emotions to a degree described as psychologic fermentation. Often the plebi reclamanti have the moral inebriation of a despot. The males of the French Revolution were largely made up of degenerates, vagabonds, criminals and fools. Lynch-law and the despotism of majorities, the opposers of the latter, the individualists and aristocrats, the relation between, and frequency of, sudden and premeditated crime, the sensibility of criminals, and hypnotic influence and crime, are discussed from the standpoint of the Morel school and Lombroso.

Le Crime et la Peine. Par LOUIS PROAL. Paris, 1894, pp. 548.

This heavy work was crowned by the academy of moral and political sciences, or rather the memoir out of which it grew was, and there is more to follow, and is printed in Felix Alcan's valuable library of contemporary philosophy. It evidently grew out of the strife between the theory of the Lombroso school that crime is a disease and the old view of strict personal responsibility, which was so bitter two years ago in France. Statistical study of crime in relation to heredity and atavism, insanity, degeneration, sex, ignorance, misery, imitation, passion, politics and free will make up the 300 pages of the first part. The second is devoted to the origin of penal justice,—responsibility and determinism. Crime is not an organic fatality, but it is progressive decay. The penal code must cling to free will against the determinists because it is useful, necessary and true. Even necessitarians, when it comes to practice, have to make of liberty an "*idée force.*"

Die psycho-pathischen Minderwertigkeiten. Von DR. J. L. A. HOCH. Ravensburg, 1891-93, pp. 427.

Reduced ability and responsibility may be innate, permanent, acquired, transient or mixed, to use the adjectives most often recurring (in this amplification in three installments of the suggestive chapter upon the same subject from the author's *Leitfaden der Psychiatrie, 1888*). The work is carefully wrought out with much casuistic material from the author's wide practice, and constitutes a valuable addition to the now so rapidly growing material on border-line phenomena.

Dégénérescence. Par MAX NORDAU. Paris, 1894. 2 vols. pp. 429, 566.

This work is dedicated to Lombroso, who is called "*un des plus superbes apparitions intellectuelles du siècle.*" None of the followers of Morel have traced degeneracy in literature. Some of the current literary modes are forms of intellectual decomposition. *Fin de siècle* is not *fin du race*, but the crepuscule or twilight of the people, and suggests the approach of a chiliastic terror, like that when the first thousand years of Christendom was passed. The *fin de siècle* French boy passing a prison where his rich father was confined for the fifth time for fraud, called it papa's lycée. The *fin de siècle* police captain has a cigar and card case made out of an assassin's tanned hide. Horrible Kate Greenaway children, Zola, Ibsen, Nietzsche, Wagner, Tolstoi's Kreutzer Sonata, Paul Verlaine, the symbolists, Maeterlinck, Sarah Bernhardt, sensations unknown to the masses,—all these are degenerative stigmata. Schopenhauer and Hartmann, graphomaniacs with incapacity to act; absence of good judgment and sound common sense, excessive impressionability, a passion for useless baubles, retinal defect, dynamogenic eccentricities, hysterical giggling, precocious adolescence and old age, jactation, mystic presentiments, pre-raphaelism, æstheticism, the salvation armies of Egidy and Tolstoi, Wagner with his delusions of persecution, the neo-Catholics, to whom Parsival is a religious service,—all these things initiate hysterical people into delicious sensations, and cause idiotic ladies to roll up their eyes and cry charming, ravishing. Hypnotism, the "fourth dimension" speculations, spiritism, psychic researchers, animal magnetism, revelation of Isis, the Ethopées of M. Péladan, the idiotic echolalia of the Belgian poet, Maurice Maeterlinck, who, like Walt Whitman, was a fool, but yet a genius, — these are described as parodies of mysticisms.

The second volume is devoted to egotism and the phobias which arise from it, its self-consciousness, Gautier, Flaubert; Mendès with his theory of "sonorities;" Bourget and the "decadents;" Metesch, the delirious philosopher Baudelaire, and Ibsen, who has become a sort of popular poet laureate, as Voltaire and Victor Hugo were. Ibsen's clientele consists of women badly married, or who feel themselves not understood, are vacuous in soul and without occupation; but he is no more their friend than is Sachez-Masach, or Zola, whose realism shows us types more fit for criminal law than for the lunacy commission; the veritists, the "young German" school, William Morris, Leopardi, Lenon, Karl Marx, Karl Bleibtreu, pessimists, and most Hegelism,—all these are degenerate. The twentieth century will be better. All these morbidities will perish, and the way to effect the cure is by the cult of unselfishness. The German "alliance of men against immorality" is to grow. On the whole, the work comes nearer drawing a line between Semites and non-Semites than any book we have ever read. Israel is still the chosen people and all others are degenerate.

Unsoundness of Mind in its Legal and Medical Considerations. By J. W. HUME WILLIAMS, of the Middle Temple, Barrister at Law, London. New York, 1892, pp. 179.

When Beccaria said: "The happiest of all nations is that in which the laws have not become a science," he hit the present state of the question of legal medicine. Common sense still needs to be heard from in determining mental unsoundness and fixing the degrees of responsibility. This should not become a purely legal question, at

any rate. It is probably impossible to fix upon any act so vicious and eccentric that, taken by itself, it could prove insanity, unless it be an involuntary result of neural disease. He must not build theories from above downward, like the architects of Laputa. Medical experts have little weight with juries, and the author would not submit questions of sanity to experts, nor distinguish very sharply between mental, moral or legal insanity. Goethe was right that nothing brings us nearer to insanity than distinguishing ourselves above others, and nothing keeps us sane better than general intercourse with many and often common people. Monomanias, moral and impulsive forms of insanity, which make most trouble in courts, are fullest treated.

Psychiatrische Vorlesungen. Von V. MAGNAN. Leipsic, Heft I., 1891; II., and III., 1892; IV., and V., 1893.

These between three and four hundred pages of the distinguished Belgian alienist contain all his more original papers, about twenty in number, and are translated into German by P. J. Möbius. Most were originally printed from students' notes. Professor Magnan, as is well known, has won his enviable reputation chiefly by his valuable work on the border-line phenomena and cases. It is impossible to do justice to these meaty papers in a brief notice. The best of them, to our thinking, are the lectures on chronic delirium with systematic evolution, which the German school prefer to call *paranoia completa*. His study of degenerate types has never been surpassed. Sexual aberrations, dipsomania, the childhood of criminals, morbid impulses to purchase things, gambling, onomatomania, intermittent phenomena, hallucinations of the right and left brain, heredity,—these are some of the special topics. The author is at his best in casuistic analysis, where, if he is not so minute as Kandinski, his penetration extends in more directions. Our own American Dr. Cowles, however, compares favorably with either of them, so far as he has published.

Ueber die Bedeutung der psychiatrischen Unterrichts für Heilkunde. Antrittsrede in Utrecht. DR. C. WINKLES. 1894, pp. 92.

The psychiatrist fights degeneration and to correct heredity. He must touch hands with the general practitioner on the one hand and with the spiritual office on the other. Degeneration on all hands, due to alcohol, opium, prostitution, anti-hygienic lives, abounds more and more. Every medical student must study psychiatry and hygiene. Doctors used to treat diseases as ontological entities, now they treat patients. They must learn to individualize; and their motto must be *minister non magister naturæ*. Not only persons but individuals are unique. Bertillon never failed to identify his man among 120,000 by the few small parts of the body he tested. Most individual is the nervous system. Psychiatry is no longer unskilled labor, but it has not yet attained due prominence in medical education. Defenses of it have been usually regarded as *oratio pro domo*. Science must not ride so high a horse that it cannot see the ground under it. Doctors have lectured at the sick bed on the anatomy of the brain, general psychology, pathology and even philosophy. Experts differ nowhere so much in court as on questions of sanity. Materialists and spiritualists as such are an anachronism. It was psychiatrists like Meynert, Forel, Gudden, Charcot and Flechsig who gave us the key to the architecture of the brain, and not the anatomists. The sick bed must not be neglected for the laboratory. The old divisions of diseases into

those of the peripheral nerves, spinal cord and brain, are obsolete—since Ramon y Cajal. The nervous system is made of superposed layers of reflex mechanism; each layer has its diseases, and each disease destroys some reflex function. Brain pathology and psychiatry must go together. With institutions for private treatment, psychiatry spoke its first serious word concerning daily life. "Normal personality" is a contradiction in terms. Each is a norm to himself. Men are not responsible for their environments or for their own organization. The root distinction of all is between hereditary and non-hereditary psychiatric forms. The psychiatrist, like the doctor, must study human degeneration in its largest aspects. The disinherited, the disequilibrated of all types and degrees are the alienist's peculiar care. He must know every type of degeneration, and collect pedigrees of all his subjects. Knowledge of symptoms of degeneration must become common knowledge. Our ancestors have left us deadly poisons as well as civilization. Wars kill the able-bodied, and weaklings are exempt. Cities rot masses of men, and charity preserves the weakest. Degenerate men and women are mutually attracted, and the stock thus happily dies out. Alcohol use, institutionalized monstrosities increase. Hygiene should teach all this. The test of science is what can it contribute to arrest this tide. The clinic is the school to know and learn to fight degeneration. Doctors should be consulted about marriage, and he should boldly oppose ill-advised unions. The clinic teaches suggestions, spiritism, subtle hysterical symptoms, that psychic blindness is simulation. Judges should see criminal brains. The ideal psychiatrist will study education,—especially that of defectives,—will be a father confessor of youth and maiden, a shepherd of souls for the sick, criminal, and for all at every important crisis of life, will see some good in the worst and some bad in the best. This is far beyond the tedious disputes about responsibility. Common types of degeneracy are hysterical cases with outer-suggestion, self-sacrificing, ecstatic, their brain cobwebs held to be real, with whimsical sympathies and antipathies, always oscillating between extremes; neurasthenics, who always procrastinate, who cannot concentrate, with anxieties, imperative ideas, frequent sex perversions and everlasting relaxation; the introverted hypochondriac, who develops illusions out of his somatic sensations; the impulsive epileptic, quick in anger, irritable and indolent, mystic and morally defective; political agitators, geniuses, etc. The hygienist, with microscope in one hand and organic chemistry in the other, must bravely war upon all these, and behind and above him must stand the psychiatrist.

Les Grands Aliénistes Français. Par le DR. RENÉ SEMELAIGNE. Paris, 1894, pp. 409.

This is the first volume in a series, and treats of Pinet, Esquirol, Ferres, Fabret, Voisin and Georget. Full page portraits of each are given. The biographies and accounts of the reforms and theories of each are well given.

Die Paranoia, eine Monographie. Von DR. C. WERNER. Stuttgart, 1891, pp. 239.

Twenty-two pages of history, summarizing all accessible literature up to date. Sixty-four pages of general matter on duration, cause, authority, prognosis and treatment, and the rest devoted to thirty special type cases, classified into primary and secondary paranoia, and the primary cases sub-divided into simple acute,

simple chronic, acute with hallucinations, and chronic with hallucinations. The secondary forms are divided into post-melancholic and post-maniacal.

Grundriss der Sprachstörungen, deren Ursache, Verlauf und Behandlung. Von DR. LEOPOLD TREITEL. Hirschwald, Berlin, 1894, pp. 100.

The author attempts to give a general view, neither too comprehensive nor too theoretic, for the general practitioner, and even germanizes many of the technical terms for the benefit of the general reader. Some points, *e. g.*, congenital aphasia and hysterical halting, are treated quite fully. Stammering and stuttering and deaf mutism occupy considerable space. On the whole the brochure adds little or nothing new, the literature at the end is very incomplete, and the eight-page chapter on the origin of language is hardly less than puerile.

Ueber Gesichtsfeld-Ermüdung, etc. Von DR. WILHELM KOENIG. Leipsic, 1893, pp. 152.

This is a laborious investigation by an assistant physician in the lunatic asylum at Dalldorf, to show the relation of retinal fatigue to the concentric shrinking of the field of vision in diseases of the central nervous system, and seems to have been prompted by the classic work of Willbrand on visual disturbances in functional nervous troubles. The latter was based on observations in an ophthalmological polyclinic, and the two together constitute most of our best material upon this topic, which is no less interesting and important than it is new. In all, data from 96 men and 118 women were used. The patient was placed, clad in black, between two windows, with his back towards them and facing a perimeter. Often two papers, each with a different color, were used. Förster's dimensions for retinal limits are assumed as normal, and tests were made for white and the primitive colors, not only on the vertical and horizontal, but in some cases on all nineteen of the radii. The casuistic material is first described in detail for both positive and negative results, in cases of simple psychic disturbance, hystero-epilepsy, chronic and alcoholic epilepsy, paralytic dementia, organic diseases of the brain, and post-traumatic diseases. Of all cases, seventy-four showed limitations of the retinal field, hysteria leading, and organic brain diseases and chronic alcoholism having least. The temporal side was more often affected than the nasal. In the course of the experiment the limitation often improved. The neurasthenic enlargement of the blind spot was also greatest at first and was more often temporal. If the limitation is monocular, it is assumed to be retinal; if binocular, it is probably central. Of the seventy-four negative cases only twenty-three showed reduced acuteness of vision.

Lehrbuch der Krankheiten der Nervensystems, für Studierende und Aerzte. By DR. ARMAND HÜCKEL. Leipsic, 1891, pp. 303, 29 cuts.

The author's ideal is condensation. This he seeks to secure by dividing his material into a general introductory part, treating disturbances of mobility, sensation, nutrition and of innervation of blood vessels, and a special part. The latter treats of the diseases of the peripheral nerves, nerve by nerve and often muscle by muscle, diseases of the spinal cord and its membranes, of the medulla, and lastly of the brain and its membranes, with a final chapter on neuroses, with neurasthenia in an appendix.

Sur un cas d'amnésie rétro-antérograde, probablement d'origine hystérique. J. M. CHARCOT. Rev. de Méd., 1892, XII., 81.

Charcot has detailed the circumstances of a most interesting case of amnesia probably resulting from an attack of hysteria, the latter brought on by the shock of bad news. The patient has wholly lost her memory of all events occurring during the month and a half preceding the attack, while since that time she has only been able to remember for a few moments what is going on around her.

Charcot believes the case to be one of modified hysteria continued under the form of amnesia. Among other reasons for this view is the susceptibility which the patient manifests to hypnotic treatment. Under a regimen of suggestion she is slowly regaining her memory. So the case is one of dynamic origin and not at all due to lesion. If the outcome is successful, as Charcot expects, the case ought to afford much valuable — if not wholly new— material. J. R. ANGELL.

Amnésie post-éclamptique. H. BIDON. Revue Médecin, NOV., 1891.

This is a brief and instructive account of several cases of amnesia caused by puerperal convulsions. The impairment of memory varies in degree from inability to recall isolated words up to the complete loss of all events connected with the pregnancy and even the married life with its preceding courtship. The most instructive case is of this latter type, to which Bidon has given the name of *amnésie systématisée*. The condition is by no means to be identified, he thinks, with cases of double consciousness, and he is confident that no symptoms are present to indicate either hysteria or epilepsy. His explanation has an attractive plausibility, and is in line with recent psychologic hypothesis. He assumes that during the convulsions the cerebral cells undergo, through profound circulatory disorder, such violent modification as to destroy all the more recent and so more superficial memory traces. The older and deeper traces of earlier events are consequently less affected. No lesion need be assumed. J. R. ANGELL.

Ein Beitrag zur psychischen und suggestiven Behandlung der Neurasthenie. Von DR. FREIHERRN, von SCHRENCK-NÖTZING. Berlin, 1894, pp. 48.

Of 828 neurasthenic patients, Hösslin found psychic symptoms in 765. The stigmata of nervous asthenia are so mobile and changeable that it is hard to fix their cause. The psyche can cause disease. Psychic cases are either (a) direct concept therapeutics, (b) indirect or negative psychic treatment, and (c) marked psychotherapeutics. Somnolence is mere idioplastic passivity, according to Van Eeden, that comes by inner concentration. Of 8,705 persons, without regard to age or sex, only 519 were refractory, 4,316 hypotaxic, 2,557 became somnolent, and 1,313 somnambulic. Out of 278 patients hypnotized by various authors, twenty-four were not helped, seventy-two recovered, eighty-two were helped, eleven relapsed. Casuistic tables presented by the author show a predominance of benefit. The enumeration of functions influenced is by far the best yet made, and shows that few functional diseases are unaffected by hypnotic suggestion.

I Problemi dell' Ipnotismo. Letture tenute all' Ateneo Trevigiano nei giorni 22 Maggio e 19 Giugno, 1892. DR. GIUSEPPE STUCCHI. Treviso, 1893, pp. 131, 12mo.

After a general and historical introduction, in which Mesmer, Faria, Braid, and later investigators are briefly referred to, Prof.

Stucchi proceeds to define and explain hypnotism, citing freely from the Nancy and the Paris schools, concluding that: (1) Women are hypnotizable in larger numbers than men; (2) youth are more hypnotizable than persons of mature age; (3) individuals habituated to passive obedience, like ex-soldiers, domestics, workmen, are more hypnotizable than persons who are independent by education and character; (4) the illiterate are more hypnotizable than those whose intelligence is cultivated; (5) those believing in the power of the experimenter are more hypnotizable than the sceptical. The author holds that the opinion of the Nancy school, that all persons are hypnotizable, is exaggerated, and that the opinion of Janet, that only those who are afflicted with nervous maladies, the hysterical, the degenerate, those who are morally or physically exhausted, are hypnotizable, is incorrect. Prof. Stucchi treats of the character of hypnosis and hypnotism, the different states and their marks, the means of producing and ending them, referring to Charcot, Bernheim, Liébeault, Beaunis, Burot, Liégeois, Ochorowicz, giving an excellent résumé. A. F. C.

V.—MISCELLANEOUS.

Psychology, Descriptive and Explanatory. A treatise of the phenomena, laws and development of human mental life. By GEORGE TRUMBULL LADD. New York, Scribner's Sons, 1894, pp. 676.

This is the seventh book and the eighth large volume put forth by the author, within the last few years, to own all of which now costs the devoted reader twenty-five dollars and fifty cents. Wundt, in a new heavy volume, fourth edition; James and Baldwin, in two volumes each; Külpe, Höffding, Dewey, Murray, Hill, Ziehen and many others; with Bascom and McCosh, perhaps, most fecund of all; new general text-books promised by Ebbinghaus and G. E. Müller, — surely in all this sudden abundance in a field where ten years ago was almost nothing, at least in English, and with all the variety of standpoint represented by these names, it seems time to call a halt, and to pray that the epoch of text-books may gradually fade into an age of special monographs in the many obscure and confused parts of this vast field. The writer of this note has now toiled through a great part of Professor Ladd's new volume, with growing marvel that he has done so much work so honestly and well. It is his best and maturest work, and contains, at least, by hint and suggestion most of the best and ripened concepts of the author's earlier works. The image of the village smith under the spreading chestnut tree, whose "brow is wet with honest sweat," etc., is the ever recurring suggestion. If there is little strikingly new, the good old story of attention, faculties, reasoning, conation imagination, impulse, instinct, feelings, etc., is clearly and faithfully and sometimes a little exiguously told. It is safe, absolutely safe, for the pupils to whom it is dedicated, and for every one else. We do not find its chief note tediousness, as Professor James does, nor over-analysis, but its all-pervading defect, as we regard it, is timidity, over-caution and conservatism, an inability, now, alas, we fear, grown hopeless, to take the clear, consistent, scientific standpoint. He has toiled nobly on toward the new city of Man-Soul, admirably portrays many of its glories, feels their fascination often deeply, but yet more deeply feels that Diabolos, if he does not still lurk in its darker by-ways, has at least not withdrawn his rear guard, and, therefore, he and his will not cross the gulf that still

lies only too fixed between. It is this step-motherly repression, this reluctance and paucity of concession, the shoals of nice scruples on which he tries to run aground the argosies of hope and promise which may be yet the best thing in a movement so vast, and so rapidly growing, — these are what come dangerously near making this a "psychology without a soul," in a sense more fatal than the author's insistent hylophobia dreams of. This is a book of the old dispensation, dignified and prophetic of, but not itself a gospel of, the new. These souls are not lost, although they die without seeing the full light. The intellect is convinced, but the heart is not converted. Nature is not yet heartily loved and trusted. The reason for this halting attitude, we believe, lies not in the author's lack of long familiarity with the practical details of laboratory and clinic so much as in a sluggishness of religious perception, a lack of prophetic insight and depth. No one has so clearly seen that the old days of opposition between faith and science—the days of Huxley's early papers, of Tyndall's prayer-gauge, of a materialism never academic, and now made obsolete by dynamism—are forever gone, and that a new sense of harmony has arisen, as shown in neo-Christians like Phillips Brooks, who boasted that he had never preached on the relations between science and religion, but always had felt them one; like Drummond, who sees in evolution only the most potent reinforcement of Christianity; like C. M. Williams, in his "Evolutional Ethics;" Paul Desjardins, and many younger men who are to shape the future. Professor Ladd can no more extract sunshine from a cucumber than he can get new religious light or heat from scientific psychology, which to an increasing number is more and more dear because big with promise for larger Christian living. These things should, of course, have no place in a text-book, but should shed a kindly light over it. Without it, we repeat, we are dealing with psychology without a soul, and the teacher is merely kindling a back fire, lest the fire of the "burning bush" spread and kindle the soul with a little enthusiasm.

These home-spun metaphors may express, at least, the present writer's sentiment toward the general spirit and attitude of the book. Its other chief defect is shared with many other text-books. The time, we think, has fully come when every psychological course, and, therefore, text-book, should at least glance at the anthropological, the morbid, the psychogenetic side. Of all three of these fields, taught every year at this university with much copiousness, there is scarcely a trace, while instinct is very inadequately treated. Unlike details concerning the senses, these lie in the scope of the book, but are simply ignored. Yet, just these are the newest and most promising lines of development. In fine, like Porter's "Intellect," this volume is a very valuable and faithfully made summary within its field, and it is there it should be judged. It contributes little that is new, and in its present bulk can do little good as a class book. The small edition which will no doubt follow, we shall await with interest.

Grundriss der Psychologie, auf experimenteller Grundlage dargestellt. von OSWALD KÜLPE, Privatdocent an der Universität Leipzig. Mit 10 Figuren im Text. Leipzig: Verlag von Wilhelm Engelmann: 1893. pp. viii., 471; Index, 472-478.

To write a text-book of experimental psychology—that is, of psychology—in the present state of the science, is a very difficult matter. This statement is, perhaps, best proved by the fact that, until the appearance of Dr. Külpe's work, there existed no text-

book that could in any sense be termed adequate: and this, although more than one psychologist of eminence had brought the matter and manner of his teaching to publication. The reason is not far to seek. Modern psychology demands a more universal training than does any other science. Its representative, in order to be a psychologist, must be neurologist, psychologist and philosopher. He must have worked over the field of experimentation, neurological, psychophysical and psychological, for himself; the three adjectives are arranged in the order of the thoroughness necessary for psychological equipment; and he must be able to psychologize, to coördinate experimental results in a system, under the light of logic (epistemology) and—though to a less extent—ontology. Not to many men is given the ability to weigh details and to generalize from them in equal measure; and not many find time and opportunity for a systematic study of four separate sciences and literatures.

Dr. Külpe is especially well fitted for the task which he has set himself. A philosopher (more particularly an epistemologist) and a teacher of philosophy, he is at the same time an experimental psychologist of great critical and constructive power. He is a pupil of the two foremost living German psychologists—W. Wundt and G. E. Müller. The *Grundriss* is dedicated to the former master, and the author's indebtedness to him is constantly apparent. But none the less certainly is Müller's influence observable, in the general, sectional arrangement of material; and in the contents of certain chapters (*e. g.*, I. 4). In psychophysics, Dr. Külpe, who has been for some years chief assistant in Wundt's Institute, has had a very exceptional experience, both pedagogical and experimental. The former has stood him in good stead in the composition of I. Chap. 1 (On the Analysis of Sensation); a chapter which is nothing less than masterly. The latter has rendered his judgment singularly good, in the most diverse departments of investigation. The vexed question as to the nature of centrally excited sensations (reproduction and association), he approaches as only one who has himself experimented in the matter can: for the section on feeling he had laid a foundation in his dissertation *Zur Theorie der sinnlichen Gefühle:* tonal fusion he is qualified to discuss by his unusual musical gifts and education: the theory of temporal association he has already handled, in a series of articles in the *Philosophische Studien:* and finally, his *Habilitationsschrift* dealt with the doctrine of will in modern psychology. Apart from these special researches, he has taken part in all the Leipzig *Arbeiten* of recent years.

A book which took shape under such conditions we should expect to be good, and Dr. Külpe's text-book is thoroughly good. Only in one regard must I confess to a feeling of disappointment: the author, despite a nominal adherence, has departed very widely from the Wundtian doctrine of apperception. His work cannot, therefore, be regarded as an intermediary between Wundt's *Vorlesungen* and *Grundzüge;* the book which shall lead from the former of those to the latter is still to be written. One can only hope, in the interests of education, that it will be written shortly. Other deviations from Wundt's system I shall notice later. There is none so important as this, though there are two or three others of primary significance.

The Introduction (pp. 1-29) falls into three sections: on the concept and problem of psychology; on its methods, and on the assistance derived by it from other sciences; and on psychological classification and literature. Psychology is a natural science; it deals with experiential data *(Erlebnisse)*, in their dependence on

the experiencing *(erlebenden)* individual. Its direct methods are the introspective and experimental; its indirect, the memorial and linguistic. The definition will, I think, be admitted to be an improvement upon those generally current. Whether memory and language can constitute methods is arguable, but Dr. Külpe makes out a strong case for them.

The body of the work consists of three parts, which treat (in Wundtian fashion) of the elements of consciousness, of the ordinary combinations of these, and of their more permanent combinations to form "states of mind." "Consciousness" is only a collective term for the sum of all *Erlebnisse* in their psychological aspect. Elementary among these experiences are two processes: sensation and feeling. These may be conjoined in two ways: by fusion and by combination. In the fusion, the constituent processes retire before the unity of the total impression; in the combination, the constituents are, in the whole, as evident as (or even more evident than) they would be in isolation. The perfect sensation has four attributes, under each of which it may be investigated: intensity, quality, duration and extension.[1] The feeling is characterized by intensity, quality and duration. It is plain that fusion occurs when qualities combine, while time and space relations remain the same;[2] whereas combination implies spatial or temporal difference. I think, with Dr. Külpe, that the two types of compounds are to be kept distinct, and that each requires a special theory (statement of conditions). Wundt prefers to regard fusion as the most intimate form of association. The difference can hardly be termed a radical one at present, since but little advance has been made towards a psychological theory of either process.

Part I. (Elements) comprises two sub-parts. The doctrine of sensation occupies just 200 pages; the whole book contains 471. Here is proof positive (if any were still needed) that experimental psychology has not stopped short at the simplest mental processes, in confessed inability to cope with the more complex. The doctrine of feeling occupies something over fifty pages. Little more than half of the work, therefore, is taken up with the consideration of the elements of mind. And a good deal of that which is included in this half might have been relegated to a later portion of the discussion, *e. g.*, the criticism of ideational associations in Chap. 4.

It is impossible here to estimate the special chapters in any detail. Chapter 1 (Analysis of Sensation) gives an admirable account of the "psychophysical measurement-methods," of sensibility and sensible discrimination. The classification of the methods is the most logical and coherent of any as yet propounded. In fact, the writing of this chapter is itself sufficient to give the author a high rank among psychophysicists. The beginner who assimilates its contents (and the subject is not an easy one to master, as all teaching psychologists know), working through and experimentally verifying the illustrations appended to the symbolical exposition of each method, will have served no inconsiderable apprenticeship in the science. In the chapter on sensation-quality, there is made an attempt to estimate the number of distinguishable qualities of tone and brightness.[3] The sub-cutaneous sensibility is divided (accord-

[1] The cutaneous sensibility has all four; the visual has quality, duration and extension; the other modalities, intensity, quality and duration.

[2] Intensive fusion may occur in the case of the paired sense organs. Here there need be no prior fusion of stimuli.

[3] No such attempt is made for colors. Of course, "color" is a fusion of brightness and color-tone. But if we start out from arbitrary saturation-values; determine their number (*e. g.*, in the solar spectrum); and then determine the number of less and

ing to the rule which classifies sensation by reference to the bodily sense-organs) into the muscular, articular and tendinous. It is certainly well to avoid the slippery term "movement-sensations." And there seems to be nothing objectionable in the ascription[1] to the tendons of a quality of strain; to the muscles of that of contraction (usually experienced in the exhaustion-complex), and to the surface of the joints, of that of locally signed pressure (*cf.* the skin). If there are missed in this chapter references to English research (*e. g.*, to the audition-theories of Rutherford and Waller), it must be remembered that the work is a German text-book, written for German students; and that, where the space-limitations are so strict, a selection has necessarily to be made. Chapter 3 is concerned with the intensive sensibility and sensible discrimination, and with the theory of the latter (Weber's law).

The fourth chapter (Centrally Excited Sensations, Reproduction and Association) is a really important contribution to systematic psychology. It is curiously Herbartian in terminology. Setting out with a rejection of the associationist dogma, "Nihil est in memoria, quod non prius fuerit in sensu," the author (1) states the problem of "recognition;" (2) inquires into the properties of centrally excited sensations; (3) subjects the associationist doctrine to a severe criticism; (4) seeks to determine the motives and tendencies, the basis and accuracy of reproduction; (5) enumerates the general conditions of centrally excited sensations; and (6) attempts a psychological "theory" of their origin. There is no "quality" attaching to centrally excited sensations, other than the qualities possessed by those principally excited; still less do the central sensations or any of them constitute a new modality. But memory is not on that account by any means identical with the reproduction of the remembered; and there are indications that the central sensations do not stand in a simple relation to the peripheral. Direct recognition consists, psychologically regarded, in a special centrally exciting efficiency (*Wirksamkeit*) of the "known" impressions or memory-images, and in a corresponding mood (*Stimmung*). The known, as such, is pleasant; the unknown, unpleasant. The known falls into place, into connection and relation; the unknown remains for some time isolated, is related by effort.[2] The development of Dr. Külpe's theory must be followed in the book itself. It is not easy reading, and the beginner would do well to familiarize himself with the ordinary associationist position, with some less penetrating discussion of "recognition" (*e. g.*, Höffding's[3] or Wundt's in the *Vorlesungen*, which is very like the author's, but more popular), and with certain of the experimental memory-researches, before attempting its digestion.

The doctrine of feeling (pleasure-pain) is expounded in a single chapter of eight sections. The author, as I have indicated above, goes further than Wundt, and makes feeling (rightly, I think, though I should prefer the abstract term "affection") an independent mental process, of equal rank with sensation. The two current

greater saturations distinguishable within each of the values so obtained:—and if we regard saturation-values and illumination-values as identical:—the attempt is not hopeless, though the result is only hypothetical. The number of color-qualities proves to be about 40 50,000.

[1] Which, however, goes beyond Dr. Külpe's treatment.

[2] To the adult mind, nothing, of course, is absolutely "unknown." At least a word will be associated to its impression.

[3] Höffding, though his theory is referred to (without a name) in the text of the chapter, is not mentioned in the list of references, p. 230.

methods of affective investigation are happily termed the serial (Fechner) and the method of expression (Mosso). Cautiously but quite definitely formulated is the view—surely correct—that it is intensity of stimulus which is effective for the pleasure-pain movement; the quality of stimulus doing no more than shift the limen of affection (or the central indifference-point) to right or left upon the abscissa of Wundt's well-known diagram. Theories of feeling are suggestively tabulated, and that of Wundt finally accepted.

Part II. (Conjunctions of Elements) also comprises two sub-parts. Fusion (some sixty pages) and combination (ninety pages) have each three chapters devoted to them. (1) The account of tonal fusion is based on Stumpf[1] (*Tonpsychologie*, II.). I notice several deviations from Stumpf's views, and cannot doubt, from my own experience, but that many others would have appeared, had Dr. Külpe been able to discuss the *Tonpsychologie* more in detail. A psychological theory he wisely does not attempt. (2) The sections on visual fusion (color-tone and brightness, based partly on Hillebrand's important paper, denying the attribute of intensity to visual sensation), etc., contain a criticism of Ebbinghaus' theory of vision. (3) Emotion and impulse are considered as representing fusions of sensation and feeling. Whether we regard the author's special conclusions and analyses as correct or not,[2] one point seems clear, that no exposition of systematic psychology, in book or lecture form, can in future neglect the doctrine of fusion in general, and of sensational fusion in particular. Just as the psychology of sensation has been elaborated, must the psychology of sensation-complexes be elaborated. When we see, our visual impressions are fusions; when we speak, our auditory impressions are fusions. There is enough opportunity for experimental work here to occupy at least a generation of psychologists.

The combination chapters discuss (1) the two psychological spaces of touch and sight. Dr. Külpe gives up Wundt's theory of the influence of eye-movement in the "construction" of the third dimension. Unwisely, it seems to me. That area is "given" we should all admit; every tactual and visual sensation is extended. But if the depth-idea is not original but associative (pp. 36, 373, 383 ff.), can not we get at it best in terms of eye-movement? For the rest, psychophysical "extension" and the Stumpf-James "bigness" (*cf.* p. 387) are quite different matters.[3] (2) Of the duration of sensations not much can be said. Of our estimation of intervals, and its conditions—thanks mainly to Meumann — a good deal. The recent revolution in the time-sense psychology will be familiar to everyone. I need not resume the author's paragraphs. He might well have given the topic more space. (3) Under the head of association proper are reviewed the phenomena of contrast (color and brightness), and of action ("reaction"). Ideational association had already been considered in I. 4. Wundt's reference of contrast to the law of relativity is rejected; Dr. Külpe tends towards a peripheral theory. My own experiments on binocular contrast appeared to me to point to the necessity of a central hypothesis; but nothing certain can at present be said. The author's theoretical analyses of the two forms of the simple reaction have already been published.[4] The compound reactions are here treated from a similar point of view.

[1] Dr. Külpe has recently stated his views of fusion in the *Zeitschr. f. Psych. u. Phys. d. Sinnesorgane*, V., pp. 360 ff.

[2] I may, perhaps, be allowed to refer to a tentative note in *Mind*, N. S., II, 6, pp. 285 ff. which I hope shortly to republish in enlarged and revised form.

[3] *Cf.* Ladd, *Psychology*, p. 326.

[4] *Phil. Stud.*, VI., VII.

Part III. (States of Consciousness) gives twenty-odd pages to attention, five to self-consciousness and will, and five to sleep, dreams and hypnosis. The treatment of attention we must examine somewhat fully.

There can, I take it, be no doubt that, for Wundt, conation (whose quality is apperception) is a "conscious" process. Apperception fuses with all the remaining conscious content; but it is itself content,—psychology deals with nothing beyond content. It can not be disentangled from the concrete to anything like the degree to which affection can,—to say nothing of the practically isolable sensation. But it is content, and we consciously-experience (*fühlen*) it as the quality "active" or "effortful." The diffused excitation consequent on the explosion of a frontal-lobe cell is paralleled by a mental process; just as the explosion of a sensory-area cell is so paralleled. If this is not Wundt's position, in the Psychology,[1] the Ethics, and the *System*, then words must surely have changed their meanings.

Dr. Külpe *a changé tout cela*. Let us put the passages together. (1) Pp. 219, 220. Apperception is a term covering "undeniable facts of consciousness." The activity of will is the expression of the totality of previous experiences. *Only a small part of it*[2] is conscious; the rest lies below the limen of consciousness. (2) Pp. 273-275. The elementary quality of will, effort (*Streben*), is reducible to definite sensation-qualities. (3) Pp. 300, 220. The "unconscious" is characteristic for the activity (*Wirksamkeit*) of fusion and attention. The "unconscious" is an "effective, but in itself not perceptible constituent of a composition of elements." The "unconscious" covers physiological processes, to which no mentality runs parallel.—The expressions are not univocal. That in giving the psychophysical conditions (theories) of certain mental processes, physiological processes, not in themselves consciously-experienced by us, must be taken into account, no one would deny (*cf.* Müller's recent work on memory). I should not affirm, either, that apperception could be "perceived" (*wahrgenommen*). The verb is only applicable to the presentation of sensations. But that there can be an *unconscious* constituent in a complex of *conscious* elements seems, in terms of the definition of psychology, impossible. The author is sailing perilously near to von Hartmannism, with all its unpsychological implications. (4) Pp. 438 ff. The "small part" of will, which is conscious, is not will at all. Introspection shows no "new conscious act" in attention; we have only "effort" (a sum of organic sensations); there is no "special content: internal activity."[3] On p. 446 *Aufmerksamkeit* and *Inhalt* are opposed; *cf.* p. 464 (*die Inhalte selbst . . . die Apperception*). The principal conditions of the appearance and persistence of attention are to be looked for outside of consciousness. Attention is not derivable from the posited elements of mind. The psychophysical process of attention is an inhibitory one. "If we combine the view that there must exist a special central organ for the operations of attention with the view that all these operations may be regarded as inhibitory processes, we obtain Wundt's apperception-theory in its most recent form." We have *Hamlet* with Hamlet left out: Wundt's

[1] In the 4th Ed., as well as the previous editions. Cf. Külpe, p. 461, with the *Phys. Psych.*, II. p. 274. Dr. Külpe's loyalty to Wundt has led him to retain the latter's phraseology; and this retention can not but prove regrettably misleading.

[2] My italics.

[3] The adjective "special" is misleading. For Wundt, the *Thätigkeitsgefühl* (= conscious experience of effort = apperceptive content proper) is a general content; but it is special, as differing from sensational or affective contents.

theory *minus* the "consciously-experienced activity, which is characteristic of the whole process of attention!" Except for the postulation of the organ of apperception (and I admit that the "except" is a large one), Dr. Külpe seems in full agreement with Professor Münsterberg. (5) Pp. 462 ff. The will-process need not be conscious. There is not necessary a third, qualitatively definite element, beside sensation and feeling. The facts of will are referable partly to the laws of reproduction, partly to apperception (*i. e.*, to unconscious physiological inhibition-processes).

Plainly enough, there is a great gulf fixed between the two theories.[1] Dr. Külpe has, for purposes of psychology, sworn allegiance to the heterogenists; though he reserves the physiological ground to autogeny. Perhaps in a second edition he will clarify his views, and give up the confusing Wundtian terminology. As things are, he seems at times to recognize a consciousness which is outside of and beyond conscious content; and to be attempting to give the appearance of fullness to a capacious old bottle, with but little new wine at his command.

In other respects, the treatment of attention is as exhaustive and clear as that of any other subject discussed in the book. I do not, however, think that it is pedagogically advisable to defer the consideration of this process till the very end of a text-book on psychology. In fact, there are several alterations in the arrangement of the contents of the work which might be proposed. The greater part of I. i. 4 should, in my opinion, be relegated to II. ii. 3. Section 40 (on the simple quality of will) does not belong in its setting. A I. iii. might have been introduced, for the consideration of apperception, without begging the question of its *Gefühltseins* or of its elementariness.[2] To explain impulse, etc., without apperception is not good psychology.[3] And minor changes might be suggested.

In estimating the work as a whole, one looks round for something to compare it with. I can find nothing but Höffding's "Outlines." There is some similarity between the two volumes. Both are published as text-books; both are compressed and matter-of-fact in style, and far from easy reading; both contain real contributions to psychology, and are not mere compilations. But there the analogy ends. Dr. Külpe's *Grundriss* stands alone as the first published complete guide to experimental psychology. And we must judge its quality to be worthy of the place in history which this fact must, of itself, assign to it.

The present review has left a vast amount of debatable matter (contained especially in the sections entitled "conditions" of such and such processes) entirely untouched. But it has already outrun its limits. It needs only to be noticed, in conclusion, that the book is well printed, on cream (not on white) paper; and that, besides the table of contents, it contains a valuable index.

E. B. TITCHENER.

Pain, Pleasure and Æsthetics. An essay concerning the Psychology of Pain and Pleasure with special reference to Æsthetics. By HENRY RUTGERS MARSHALL. Macmillan & Co., New York.

The new inductive psychology, which began with laboratory experiments upon the senses, reaction-times, and the psycho-

[1] For deviation from Wundt on a point of detail, which seems due to a misunderstanding of him, see p. 464.

[2] Which latter, however, seems granted; p. 452, § 75, 1.

[3] *Cf.* Mind, *l. c.*, and Külpe's own analysis, for the justification of this. He has, of course, no room for a fusion of affection with conation; *cf.* p. 446.

physic law, has been for a decade drifting on toward the study of the active powers of will, and there are many very recent signs that it is entering the still larger field of feeling and emotion. When it does so, it will cover the entire ground of man's psychic life. As the problems have deepened, the old materialistic bias of these studies has decreased, until there now seem promise and potency of deeper insight even into man's religious life. Mr. Marshall's book is thus most opportune, and contributes so much clearness, confusing as is his plan of arrangement, that it must be read by every one interested in the subject. He has made himself well acquainted with the vast and varied literature of the subject, save only the works of Oppenheimer — which probably appeared just too late — and of Dr. Henry Head, both of whom discuss the problem of pain in a way that is very important for his theory. Besides being severely scientific, they base their work more entirely upon anatomical and pathological data than Mr. Marshall approves.

We have read Mr. Marshall's book from cover to cover, and are much indebted to him. His fundamental position is that pleasure and pain are not the basis or raw material out of which all mental life is developed, because were this the case pleasure and pain would be used up, like raw material, in the product; and if mind was made out of them, it would show traces of their duality. Neither does he think pleasure and pain to be *sui generis* and apart, like special senses; his view is that they enter as differential qualities into all mental states, and that either of them may belong to any act or element of consciousness. If they are special qualities, they may come to all mental phenomena. Mr. Marshall's classification of instinct-feelings, of which the emotions are complexes and coördinates, is clear and convenient. Joy is a complex psychosis of coming advantage; dread, of disadvantage. Sorrow is loss of advantage; relief, of disadvantage. Over against these four passive are four active feelings—love, a complex psychosis tending to go out to beloved objects; fear, tending to flee from disadvantage; anger, to drive it away; and surprise, which is a concentration of effective action on a single object. To these last four he adds a tendency to imitate, and another to please or attract advantage. States of pleasure and pain, or algedonic states, to use the author's convenient new term, color all and do not have the wide neutral or untoned interval between them which Wundt—whom the author thinks is coming around to his general view—urges. Due scope is given to the nutritive factors which Grant Allen first brought into prominence. The description of each emotion is interesting and comprehensive. Although emotion is said not to originate in reflex or other movement or attitude, due attention is given to the latter. We could, however, but wish that so competent an author could have included the fascinating topic of sign-language, and perhaps even Delsarte, in his field of view. The object of art and pedagogy might be conceived as the enlargement of pleasure-fields — to use another happy conception of the author—and the frequency and prolongation of pleasure-states. Algedonic æsthetics are thus related to pedagogy and to ethics, and racial pleasure-getting is equivalent to racial effectiveness. This view does not favor utilitarianism nor egoistic hedonism. Even the bitterest restrictive pains should not be eliminated, for this would be death of the higher entity. The relatively permanent pleasure-field of revival is for each person the æsthetic field to which he refers in making judgments.

LETTERS TO THE EDITOR.

DR. FOREL, OF ZÜRICH.

TO THE EDITOR:

The following short account of the work of Dr. Forel is gladly sent in response to your request:

Outside of Switzerland, Dr. Forel's reputation is based largely on the fact that he is one of the greatest authorities on hypnotism, but a more intimate acquaintance with his work here brings other phases of it into equal prominence.

In appearance Dr. Forel is large and commanding, and shows in every movement the spirit of activity that is one of his most marked characteristics.

The medical department of the University of Zürich has attained a good reputation among German universities, and when vacancies occur, effort is made to secure the best men from abroad to fill them. A few years since Dr. Forel was called from Munich to occupy the position of professor of psychiatry in Zürich. In connection with this office, he gives two courses of lectures during the year, one on general psychiatry, and one on hypnotism, and also holds semi-weekly clinics. His lectures are very popular and well attended, and in the course on psychiatry he touches on many general subjects, which show how broad and far reaching his interests are. The opening lecture states his *Weltanschauung*. He is a pantheistic monist, believing in the existence of God as a universal force, of which life and matter are manifestations. He denounces with great vigor all anthropomorphic ideas of a personal God, and explains what is commonly called sin as being the result of a weak *Keimplasma*. He gives one lecture of the course on Weismann's theory of heredity, which he accepts. Among the most interesting of his lectures this year were one on brain localization, and one on disturbances of speech.

The clinics are attended by medical students, and, by special permission, by students of psychology. To the latter, these clinics furnish a fine opportunity for the study of pathological psychology, as Dr. Forel never fails to make the psychological significance prominent in the diagnosis of the cases examined. Among the rare cases presented this year was that of a patient suffering from paragraphia, with general paralysis of the muscles of the eye, on the same side as the brain trouble, causing the paragraphia. Only one other case of the kind has ever been known. Also a case of negative hallucinations of memory, the possibility of which was discovered a few years since by Bernheim, and several interesting cases of auditory and motor hallucinations.

In addition to his official position as professor of psychiatry, Dr. Forel is director of a large insane asylum in Burghölzli, a picturesque village about two miles from Zürich. Here he holds his clinics,

and here is his laboratory, which contains the largest collection of pathological brain material in Europe. He is most cordial to all who visit the asylum for scientific purposes, and very free in offering opportunities for study in his laboratory. Dr. Forel himself is making some special investigations in brain localization, and is quoted as authority in scientific works on brain anatomy. As director of the asylum, Dr. Forel is noticeable for the philanthropy which enters into all his relations with the insane. He studies to promote the comfort of even the most abandoned cases, and often patiently listens himself to the details of slight disagreements among them. His regimen for the patients is liberty and work, in so far as possible.

Dr. Forel's investigations in hypnotism are well known. His work on hypnotism is much superior to that of Moll, and is probably the best hand-book on the subject yet produced. He is also one of the editors of the *Zeitschrift für Hypnotismus*, published at Berlin. In his theories of hypnotism he belongs to Bernheim's school, although he opposes all dogmatism on the subject. He insists on accurate scientific methods in hypnotic research, and denounces the large class of psychologists who condemn hypnotism on *a priori* grounds, without having any experimental knowledge of it. His method of inducing hypnosis is short and quick. He causes the subject to look him fixedly in the eyes. Then, holding his thumb and finger before the two eyes of the subject and near, he bids him look at them, and drawing them down and away, the eyes diverge, and at the same time close, and the subject is asleep. On subsequent occasions a mere "*schlafen Sie*" is usually sufficient. The percentage of hypnotizable subjects Forel puts very high.—I think from 90 per cent. to 100 per cent. among normal persons. Despite the fact that an insane asylum is the most unfavorable place for the practice of hypnotism, Dr. Forel furnishes remarkable examples of its psycho-therapeutic value, and striking illustrations of the power of post-hypnotic suggestion. One cannot but think, however, that his success in the latter is due largely to the influence of his own strong personality. One feature of his use of hypnotism is his economy of labor in the insane asylum by its means, a course which has been opposed by his confrères, but the practical advantages of which they have been obliged to admit. One occurrence of the past year will illustrate this. An insane woman was expected to give birth to a child, and as on several previous occasions she had caused the death of her infants, the closest watch was required. Instead of providing night watchers, Dr. Forel hypnotized a waiting maid, and put her to sleep in an adjoining room, telling her to sleep quietly, but to awake and call the doctor should the woman be taken ill in the night. The waiting maid slept, undisturbed by the ordinary screaming of the insane woman, but awoke the moment that medical aid was required, and the life of the infant was thus saved.

In the city of Zürich Dr. Forel is well known as the president of the Temperance Society, a prominent member of the Society for Social Purity, and the leader in a movement to procure funds for the establishment of a People's Palace. He advocates total abstinence from alcoholic drinks with great vigor, and does not allow any use of alcohol in the insane asylum. He never fails in his clinics to emphasize the fact that a large proportion of the cases of insanity result from the prevalent use of *Schnapps* in Switzerland, and one of his most prominent lectures is a vigorous setting forth of the effect of alcohol on the brain tissue.

This short outline of Dr. Forel's work would not be complete without reference to his investigations in natural science. Among other studies in this direction, he has worked for years in collecting material in regard to the habits of ants. His library is full of books on the subject, and his published articles about ants and their habits are authoritative. MARY MILLS PATRICK.

Zürich, April 20, 1894.

A CORRECTION.

EDITOR AMERICAN JOURNAL OF PSYCHOLOGY:

Sir:—In the last number of the AMERICAN JOURNAL OF PSYCHOLOGY, pp. 228-9, Mr. T. L. Bolton courteously gives me credit for suggesting the rhythm instrument there pictured and described. In doing so, however, he has fallen into error. The rhythm instrument is but a slight modification of a beat instrument used long ago by Prof. Alfred M. Mayer (*American Journal of Science*, 3rd Ser., VIII., 1874, 241; and XLVII., 1894, 5).

E. C. SANFORD.

The editors of *Mind* request that all MSS. from America, intended for publication in that journal, be forwarded to Prof. E. B. Titchener, Cornell University, Ithaca, N. Y., instead of to Mr. Stout, as has hitherto been the case.